IPCC Working Group I Fourth Assessment Report

Expert Review Comments on First-Order Draft

Chapter 3

Batch AB

Notes

The following expert review comments are for consideration by the chapter writing team and require a brief response to each comment. This document with completed responses should be returned to the IPCC Working Group I TSU. The authors' responses will be retained in an open archive on completion of the Report for a period of at least 5 years.

Instructions for managing review comments on the First- and Second-Order drafts

The chapter writing teams should consider all expert review comments and record their agreed responses in this document. Coordinating Lead Authors may wish to organize discussions of general comments, and ones with broad implications for the chapter, among the whole author team. Responsibilities for responding to specific comments may be allocated to appropriate authors within the team. This document may be used to note which member of the author team is responsible for implementing a particular response to a comment, but all recorded responses will be taken to represent agreement of the team as a whole.

In most cases responses should be brief but the action taken in response to the comment should be unambiguous. For example:

- **Accepted** - with or without comments (e.g. in case of minor modifications)
- **Rejected** - with motivation
- **Taken into account** - with explanation (e.g. combined with other comment / covered in other section)
- **Noted** - with or without comments (e.g. in case of remarks, where no text revision is required)

Responsibilities of the Review Editors

The responsibility of a Review Editor is to ensure that all substantive expert and government comments are given appropriate consideration by the authors. The Review Editors may advise lead authors on how to handle contentious/controversial issues and should ensure that genuine controversies are reflected adequately in the report. Review Editors will participate in the Third and Fourth Lead Author meetings.

No.	Batch	Page:line From	Page:line To	Comment	Notes
3-1	A	0:0	0:0	The whole chapter contains a lot of good research results since the TAR. While it is impossible to include everything, the authors have done an excellent job in finding reference material. I don't think there can be too many references in such a report, and in fact I have a few more to add if the authors would consider them. Also, in such a work balance is highly appropriate. I applaud the authors, and now it's time for the reviewers to do their job. This chapter also represents a nice primer on the current state of the General Circulation. I am happy to contribute as a reviewer. [Anthony Lupo]	
3-2	A	0:0	0:0	The authors are to be commended for having produced an excellent chapter that presents a tremendous amount of important new material very well. My comments here are intended largely to help the readability for the non-expert reader. [Susan Solomon]	
3-3	A	0:0		I'd like to see all the trend values expressed in ºC as it is expressed, for example, in figure 3.2.9 [Enric Aguilar]	
3-4	A	0:0		Comments: Why some points of measurements (observations) far for urbanisation such as Assekrem (2750m) in the Hoggar (Algerian Sahara) are not considered to show the real climatic change in temperaure, [Matari Amar]	
3-5	A	0:0		The chapter is comprehensive and very detailed but sometimes hard to read due to the many acronyms. This is perhaps for the most part unavoidable, but in those parts that are likely to be read by non-experts (summaries, boxes, Questions etc.) an attempt should me made to avoid acronyms. [Fons Baede]	
3-6	A	0:0		Answers to Questions often refer to information drawn from different sections of this Chapter. I therefore suggest to place all Questions at the end of the Chapter [Fons Baede]	
3-7	A	0:0		Each subsection should begin with a clear quantitative summary of the results; where possible, in the form “x = X0 +/- dX”, or “ no consensus exists on. . .” [Marcia Baker]	
3-8	A	0:0		The Questions (3.1 and 3.2) appear at surprising places in this chapter. [Marcia Baker]	
3-9	A	0:0		There should be clear statements wherever possible of differences between current results and those in TAR. [Marcia Baker]	

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3-10	A	0:0		The color palettes are not well chosen for the maps (Figures 3.2.9, 3.2.10, 3.3.2, 3.3.3, 3.4.4) [Marcia Baker]	
3-11	A	0:0		The section dealing with the two long term GPCC data sets is somewhat mistakable. Things would maybe clarified by replacing this passage with: „The Global Precipitation Climatology Centre (GPCC) provides two monthly data sets from surface gauges on 0.5 , 1.0 and 2.5 grids. Both data sets have been constructed using several GPCC sources (including data from CRU, GHCN and FAO and many nationally data sets provided from more than 170 nations). While the data set designated GPCC VASClimO (Beck et al. 2005) uses only quasi-continous station time series that have been subjected to homogeneity testing and homogenisation and as well detection and elimination of outliers in order to improve temporal homogeneity the GPCC Full Data set (updated from Rudolf et al. 1994) uses all available stations to provide more complete spatial coverage in each individual month.“ Reference for Beck et al. 2005 – Beck, C., J. Grieser and B. Rudolf (2005): A new monthly Precipitation Climatology for the global land areas for the period 1951 to 2000. Climate Status Report, 2004: 181-190, German Meteorological Service – available via http://www.dwd.de/de/FundE/Klima/KLIS/prod/KSB/ksb04/28_precipitation.pdf [Christoph Beck]	
3-12	A	0:0		Why is the study Harries et al. (2001) 'Increases in greenhouse forcing inferred from the outgoing longwave radiation spectra of the Earth in 1970 and 1997', Nature, Mar 15;410(6826):355-7 cited & discussed? [Rasmus E. Benestad]	
3-13	A	0:0		There are many statements through the chapter about limitations in the observational data base. It would be useful to add a section at the end of the chapter pulling this information together and listing what is needed to improve the quality and consistency of the observational data base. Such a listing would support the concerns voiced by GCOS and others about the deterioration of the climate observing system and its potential impact on our ability to both understand present climate and project future climate. [Lenny Bernstein]	
3-14	A	0:0		TSU NOTE: Please see supplementary review material [Reinhard Boehm]	
3-15	A	0:0		Thoroughly reading the whole chapter 3, I find out that I do not have too much to add or comment for improvings. The structure of the chapter covers all aspects of observation data. If any restrictions on page numbers is not required, Chapter 3 of AR4 will be a comprehensive report on surface and atmospheric climate change based on observations since TAR.	

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				[CONSTANTA-EMILIA BORONEANT]	
3-16	A	0:0		New References from our comments: Bromwich, D.H., A.N. Rogers, P. Kallberg, R.I. Cullather, J.W.C. White, and K.J. Kreutz, 2000: ECMWF analysis and reanalysis depiction of ENSO signal in Antarctic precipitation. J. Climate, 13, 1406-1420. Fogt, R.L. and D.H. Bromwich, 2006: Decadal variability of the ENSO teleconnection to the high latitude South Pacific governed by coupling with the Southern Annular Mode". J. Climate, accepted. L'Heureux, M.L. and D.W.J. Thompson, 2005: Observed relationships between the El-Niño / Southern Oscillation and the extratropical zonal-mean circulation. J. Climate, in press. Turner, J. S.R. Colwell, G.J. Marshall, T.A. Lachlan-Cope, A.M. Carleton, P.D. Jones, V. Lagun, P.A. Reid, and S. Iagovkina, 2005: Antarctic climate change during the last 50 years. Int. J. Climatol., 25, 279-294. [David / Ryan Bromwich / Fogt]	
3-17	A	0:0		Excellent synthesis of the enhanced knowledge on surface and atmospheric climate change. Congratulations [Manola Brunet]	
3-18	A	0:0		The chapter is well written, and also the choice to stress the most recent period is reasonable. But some more efforts should be spent to discuss also the earlier instrumental period: many efforts were made in the last years to improve data coverage and data quality for that period, and many projects were involved in this sense (ADVICE, EMULATE, IMPROVE, ALPCLIM, ALP-IMP, only as far as the EU-funded projects are concerned). [Michele BRUNETTI]	
3-19	A	0:0		Chapter 3 is a very long review of atmospheric change with emphasis on the past 5 years since the TAR. As an oceanographer I was not familiar with most of the topics so my comments are those of an interested scientist rather than a specialist in atmospheric processes. I enjoyed reading Chapter 3 and I learned a great deal about the changing atmosphere. [Harry Bryden]	
3-20	A	0:0		From my reading of the entire Chapter 3 and from the Executive Summary, most of the specific results on climate change are in sections 3.2, 3.3 and 3.4 in the first half of the Chapter where the "trends" are carefully stated and examined. The second half of the Chapter seems to be about changes that are more likely to be associated with long-period variability. The latter half of the Chapter was fascinating to read, but generally	

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				inconclusive. Could it be shortened to make the overall chapter more accessible to read for the major points in the Executive Summary? [Harry Bryden]	
3-21	A	0:0		I was impressed with the description of the global warming, the increase in water vapour content, trends in DTR, tropospheric warming, stratospheric cooling. Each was carefully defined, statistically analysed, patterns reported. The tone of the chapter changed at 3.5.3 "Storm Tracks" which struck me at the time as poorly defined phenomena that were hence difficult to assess for their changes over time. The ill-defined topics continued with Blocking, Winds, Teleconnections, Monsoons, Extreme Events. They are all fascinating topics that provide the working language of climate scientists. But will we ever be able to assess whether they are changing over time until we have a proper definition of them. Hurricane research seems to be making progress with definitions of total destructive power that can be quantified and analysed for long-term trends. Do we need specific definitions for these phenomena in order to assess whether they are changing in a warming climate? [Harry Bryden]	
3-22	A	0:0		I was most concerned about the "climate shift" of 1976 that is mentioned repeatedly throughout the Chapter but never with an explanation of what it was. I came away with the impression that the most important climate change had occurred a long time ago and the last 5 years, indeed the last 30 years, were just some small drift in temperatures. What was the "climate shift" of 1976 and how does its magnitude and importance compare with the clear trends in temperature, water vapour content, DTR? [Harry Bryden]	
3-23	A	0:0		The chapter is very well prepared but some sections are too extended with a few repetitive passages and too many details. Some findings could be better summarised. [Aristita Busuioc]	
3-24	A	0:0		The areas pertinent to my expertise, surface and upper air temperatures, in FOD are considerably improved over those in ZOD. My complements to the Lead Authors, B. Soden and D. Parker along with their convening Lead Authors, P. Jones and K. Trenberth for the improvements. The difficulty with this section is the fairly rapid pace of change regarding assessments of the current datasets and the appearance of new datasets. It is likely that important information will appear soon after the AR4 goes to press that will put the AR4 out of date. I've seen a few comments about this unfortunate circumstance, but it may behoove the authors to include a clear statement about the burden of dealing with the evolution of knowledge concerning the various aspects of observed climate change as reported on in this chapter. I will focus on areas I think need improvement, which I believe are the fault of the evolving science, and not a bias of the authors.	

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				[John R Christy]	
3-25	A	0:0		As a general comment on chapter 3, I find it very well organised and exhaustive. Perhaps a better effort could be done to make it more easily readible, by reducing the very large amount of data collections that are reported in the various sections, limiting to those which showing more clearly a trend or an effect of climate change, and citing more sinthetically the others. Also some repetition could be avoided. Questions 3.1, 3.2 and 3.3 are a very effective way to condense in a few sentences the relevant information from each topic, perhaps they could be extended to other chapter's sections. Although the list of references is very large and accurate, since the interested reader may be not acquainted with all physical phenomena and terms used in the chapter, it may be worth to introduce some term with a short description or explanation (specially for chapters 3.4 to 3.6; as ana example, the term "annular mode"). [Franco Desiato]	
3-26	A	0:0		Throughout this chapter is a perhaps common but inaccurate use of English. That is "trends" are described as increasing (i.e. second derivative) where the intent is to say they are positive. Likewise decreasing is used to denote negative trends, or other similar words that confuse information on sign with information on derivative of trends.. [Robert E. Dickinson]	
3-27	A	0:0		The authors have overall achieved great success to assess comprehensively and in a balance way the important advances and developments on the observations of surface and atmospheric climate changes since the TAR. This chapter is a fair reflection of the current state of scientific understanding of the related issues. [Qiang Fu]	
3-28	A	0:0		The chapter is very well organized and documented. [Savitri GARIVAIT]	
3-29	A	0:0		The connection with the previous chapter should however be emphasized, e.g. how RF is related to surface temperature, or how RF contributes to surface climate, etc. Actually, this will help scientists and policy makers of developing countries, especially those located in Asia, where the emissions of RF agents are high, getting prepared by enhancing capacity building and formulating appropriate control strategies. [Savitri GARIVAIT]	
3-30	A	0:0		Generally I think this chapter is in good shape, and I learnt a lot by reading it. I did not notice any large inconsistencies with the contents of chapter 9 (of which I'm an LA), except perhaps that we are more cautious in the attribution of climate change to particular causes in some cases. I don't remember the target length for this chapter, but I suspect that the chapter must be shortened. In this case, I suggest that some of the sections which describe features of the mean climate or variability (as opposed to climate changes) might	

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				be shortened. [Nathan Gillett]	
3-31	A	0:0		Very impressive chapter, with a lot of information. As for the whole report, some reduction, particularly for the background material, would enhance its impact. [FILIPPO GIORGI]	
3-32	A	0:0		I found the discussion of global warming in the 20th century a bit confusing. In the TAR, the public has been given the message that the 20th century warming number was 0.6 C. This number does not seem to have changed in the AR4. However also the number 0.75 C is mentioned concerning warming to 2004. It is unclear, however, if this is only due to further warming from 2000 to 2004 or to the filtering of the time series. Since this will be a key number to quote, this issue should be better clarified. In other words, why is the 20th century TAR warming of 0.6 now becoming 0.75? Please be clear about his. [FILIPPO GIORGI]	
3-33	A	0:0		The arrangement of paragraphs in this Chapter is distorted in order to place an unjustified emphasis on one aspect of the surface record, that from weather stations and ships (1901 to 2004). and its supposed "linear trend'. You report a large number of different measurements of global temperature change, all of which need to be seriously considered. Undue emphasis is placed on the limitations of all the records except the surface record. The limitations of the surface record are omitted altogether. All the records of global temperature change should be together in one section, not separated as at present to give the priority to the surface readings. This will involve changing the order of the paragraphs. And including the evidence that the surface readings are themselves subject several sources of error [Vincent Gray]	
3-34	A	0:0		The "corrections" to the surface temperature record have always been based on very poor evidence. The many references to studies on individual or regional stations which find the need for much higher corrections than are currently applied, are ignored. Now you have ignored the persuasive evidence of McKitrick and Michaels 2004 Climate Research 26 156-173 who have shown a significant influence on your "corrected" figures of a series of socioeconomic factors. You cannot just ignore this paper. [Vincent Gray]	
3-35	A	0:0		chapter overall: Reads well and balanced, I enjoyed reading it and found it very interesting. The figures are well chosen and nice. [Gabriele Hegerl]	
3-36	A	0:0		Some figures end quite a bit before 2005, any hope of updating them? [Gabriele Hegerl]	
3-37	A	0:0		Some sections are a bit technical, such as the one on radiative changes, and on	

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				evaporation, but overall it reads well. [Gabriele Hegerl]	
3-38	A	0:0		In some places, I had a hard time following the flow, so more cross refsin the text would be good, such as from overall precipitation to heavy precipitation, and between the various sections discussing changes in circulation NAM SAM etc. [Gabriele Hegerl]	
3-39	A	0:0		My main critiques of this chapter's treament of evaporation, evapotranspiration, and atmospheric water vapour resolve into the following categories: (i) confused terminology; (ii) confusion about the relationships of the various evaporative fluxes; (iii) the inconsistent treatment of solar radiation forcing on said fluxes; and (iv) the consequent misestimation and poor prediction of drought. The following four comments (#2 to #5) expand on these categories in turn, and most of the line-specific comments following them (#9 to #79) highlight their instances in the text of the chapter. In my comments, I use the following abbreviations: ET = evapotranspiration in general; ETa = actual ET; ETp = potential ET; Epan = pan evaporation; Tair = air temperature; Tdew = dew-point temperature; VPD = vapor pressure deficit (i.e., saturated vapor pressure less actual vapor pressure); and RH = relative humidity. [Michael Hobbins]	
3-40	A	0:0		(i) terminology: as it relates to evaporation, the terminology in this chapter is all over the place and, I think, sometimes reflects the confusion in the writers' minds as to what flux they're actually talking about and how different types of evaporative fluxes relate. [Michael Hobbins]	
3-41	A	0:0		(ii) relationships of fluxes: the Epan flux and regional ETp measure are complementary to the ETa flux. Epan meters ETp, and ETp is complementary to ETa on a regional scale. In the absence of trends in the radiative dynamic (net available energy), trends in the aerodynamic driver (humidity and windspeed) force ETp and Epan to vary in a complementary manner to ETa: the flux rates converge as moisture availability increases and diverge as moisture availability decreases. Radiative trends force all ET fluxes and measures (Epan, ETp, and ETa) to respond in the same direction as the radiative trend. When there are trends in both dynamics, the responses in the ET fluxes and measures superpose. The theory, modeling, and observations of this complementarity are all well documented: *Bouchet, R. J., 1963: Évapotranspiration réelle et potentielle, signification climatique. International Association Scientific Hydrology, Proceedings, Berkeley, California, USA, Symp., Publ. No. 62: 134–142., *Morton, F. I., 1983: Operational estimates of areal evapotranspiration and their significance to the science and practice of hydrology. Journal of Hydrology, 66: 1–76.; *Brutsaert, W., and H. Stricker, 1979: An advection-aridity approach to estimate actual regional evapotranspiration. Water	

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				Resources Research, 15(2): 443–450; *Brutsaert and Parlange, 1998; *Hobbins, M. T., J. A. Ramírez, T. C. Brown, and L. H. J. M. Claessens, 2001a: The complementary relationship in estimation of regional evapotranspiration: the CRAE and Advection-Aridity models. Water Resources Research, 37(5): 1367–1388; *Hobbins, M. T., J. A. Ramírez, and T. C. Brown, 2001b: The complementary relationship in estimation of regional evapotranspiration: an enhanced Advection-Aridity model. Water Resources Research, 37(5): 1389–1404; *Hobbins et al., 2004; *Ramirez, J. A., M. T. Hobbins, and T. C. Brown, 2005: Observational evidence of the complementary relationship in regional evapotranspiration lends streong support for Bouchet's hypothesis. Geophysical Research Letters, 32, L15401. [Michael Hobbins]	
3-42	A	0:0		(iii) Solar radiation forcing: Solar radiation is a more direct driver of all evaporative processes than is temperature, providing the entire energy input to the process, and it is one in which we know there have been significant trends over the last few decades. Yet this chapter makes frequent reference to warming driving ETa upward, without mentioning the more significant (and generally downward) signal in ETa due to trends in solar radiation. It is this sort of simplification and disconnect that leads to the point (iv). [Michael Hobbins]	
3-43	A	0:0		(iv) Drought prediction and estimation: This chapter's drought predictions are predicated on Dai et al.'s (2004) use of the Palmer Drought Severity Index (PDSI) to estimate a 132-year drought series. Drought measured by PDSI implies (i) that the soil evaporates at ETp until evaporation ceases (Palmer's original simplification for ETa); and (ii) a solely temperature-based ETp dynamic (the Thornthwaite expression for ETp). Both assumptions are outdated (1965 and 1948, respectively) and, in particular, hydrologists have fought long and hard to point this oversimplification out, so its inclusion at this stage in the game is a disappointing retrograde step. As for the case that ETp increases with Tair, this is another gross and misleading oversimplification. Take the case of the conterminous US: over the past 50 years, even as Tair has increased (~ 0.1degC/decade), Tdew has increased over twice as fast (~0.28degC/decade). The decreasing difference between Tair and Tdew is, of course, reflected in the VPD, which is decreasing "from below" with actual vapor pressure rising faster that saturated vapor pressure, not increasing "from above" as in the Thornthwaite/PDSI paradigm. Thus the drying power of the air is also decreasing, tending to drive ETp down. That is besides the effect of solar radiation, which is is a more direct driver of all evaporative processes than Tair, providing the entire energy input to the process, and in which we know there are significant trends. Together, decreasing net available energy and decreasing drying ppower of the air add up to one thing: decreasing ETp, which is observed as decreasing Epan (Roderick and Farquhar, 2002; Hobbins et al., 2004; Roderick and Farquhar 2002xxx; Roderick and	

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				Farquhar 2002xxx; Chen et al. XXX). Simply put, warming does not imply drying. This over-simplification/error is endemic throughout this chapter, and it must be corrected, as it has significant policy implications (in the TAR, policymakers were advised that warming-driven drying had been observed in the mid-latitude continetal interiors over the past half-century, and would lead to increased drought exposure in the future). [Michael Hobbins]	
3-44	A	0:0		There is a distinct imbalance in citations (17 1st-author citations for Trenberth--the most by far--nine by Dai, the third-most). Whilst I understand that one knows one's own work--and perhaps that of one's colleagues and/or students--the best, over-reliance on said work necessarily leads one to over-stating one's own paradigm. At best, this precludes proper synthesis of the diversity of opinion on a subject; at worst, it leads to one's own incorrect assumptions and errors being compounded and transmitted through repetition. I think that is somewhat responsible for the fundamental errors in drought prediction in this chapter, about which I have said more below. [Michael Hobbins]	
3-45	A	0:0		In summary, this seems to be the thinking implicit in this chapter: "ETp is a function of temperature, therefore, when Tair increases with global warming, ETp increases. Thus, ETa increases, therefore we have drying (the "brownhouse" effect). Contradictory observations of decreasing Epan mean that Epan cannot be trusted to tell us anything about ETa, which is, in any case a function of surface wetness alone." Thus is the regional complementarity of ETp and ETa ignored in this chapter. Thus are the effects of global dimming on all evaporative fluxes ignored. Thus are model estimates (ETp by Thornthwaite) preferred over real-world observations (Epan) of the flux under discussion. Thus is Epan de-linked from ETp, ETp de-linked from solar radiation, and ETa linearly linked to ETp. Even when the report avoids this over-simplistic paradigm—summarizing results that include radiation in the estimation of ETa, for example—it falls right back into it by then discussing them in the context of Tair alone. This chapter, therefore, needs a careful review in the light of—as a start—correcting the brownhouse assertions, cleaning up the terminology, acknowledging explicitly that its use of ETp in its drought estimates excludes variations in solar radiation and Tdew, and re-framing the trends in all evaporative fluxes in the context of their observed complementarities. From within such a framework, all the observations to which this section refers—including those relating to Epan—can be seen to paint a coherent hydrologic picture. [Michael Hobbins]	
3-53	A	0:0		In the chapter 3 the description of trends doesn't pay so much attention on significance of the trends. There are listed trends and changes per decades but there is little information about their significance. If it is not on the $p<0.05$ level, we can consider them as	

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				insignificant. This question is the most important in case of short-term trends, for example during 1979-2004. I expect the most of these trends are insignificant during such a short time interval. Insignificant trends may express only short-term climate fluctuations that will disappear when we change a bit the period of trend analysis. [Jaak Jaagus]	
3-54	A	0:0		I suggest that all the authors should be listed in the list of references of the chapter 3, not only the first author et al. [Jaak Jaagus]	
3-55	A	0:0		Some authors referred too many of their own papers, which make the list unbalanced. I suggest each author shall have maximum fewer than three papers being referred. As IPCC is to show all contribution from the community. [Menglin Jin]	
3-56	A	0:0		All submitted papers need to be removed from IPCC. [Menglin Jin]	
3-57	A	0:0		The chapter discussed limitations in the observational data base in many sections. A section should be added to the chapter compiling this information, discussing its cumulative impact on our ability to understand current climate and project future climate, and suggesting ways in which the collection and analysis of climate data could be improved. IPCC has a responsibility to policymakers to provide such as assessment. [Jeffrey Kueter]	
3-58	A	0:0		This Chapter is impressively dense and diverse. Indeed, it includes multiple concepts, strategies, and approaches to the climate assessment process. I found this section challenging to review, simply because some of the key points of logical connection that I found confusing, or omitted, show up in later Chapters that I have not yet reviewed. The first four sections deal, as expected, with the "classical" approach to climate trend monitoring--employing as much space- and time averaging as possible to achieve robust conclusions concerning evaluation of climate-forcing induced trends of temperature, precipitation, and DTR(Oops!, I have just introduced one of the zillion undefind acronynms.) Clearly, a "box" of defined acronyms is needed for all but the most obsessive readers of this dauntingly longest of all Chapters. Importantly, the sharp discontinuity between Sections 3.1-3.4 and Sections 3.5-3.9 have very small points of meaningful contact. The care and rigor employed in the "classical" analysis of Sections 3.1-3.5 is very impressive, and gets to the very heart of the required data, and its diagnoses, to capture the very essence of the evidence for the presence of human-induced climate warming. Sections 3.5-3.9 do a very nice of demonstrating how difficult it is to separate regional climate "change" from regional natural climate variability. Unfortunately, the reader does not receive a convincing explanation of why it is	

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				discussing all of these acronynmd "modes" of interannual to decadal to century-scale natural variability. It is clear that a better mechanistic understanding is required of the role of these "modes", and their decadal-scale roles in shaping regional-scale climate anomoly "predictability", if any, in the detection-attribution problem. Simply describing these "modes" in a general way does not answer the greater challenge in a particularly convincing way. The challenge may be made simpler and more convincing if, somehow, the use of climate model ensembles(hopefully utilized in a constructive way in Chapter 9, which I will check later) to achieve a better statistical/physically based set of insights into how our planet is responding to its human-induced climate forcing mega-problem. I do, however, recommend that the Co-ordinating Lead Authors re-evaluate the lengthy discussions of Sections 3.5-3.8 in the light of the complementary model-ensemble-based discussion in Chapter 9. [Jerry Mahlman]	
3-59	A	0:0		I think that it is appropriate to acknowledge that Chapter 3 provides the core of the accomplishments of WG1. Because of this, I assert that it is imperative for the Executive Summary be written as absolutely carefuly as possible. Clearly, accomplishing this will almost guarantee that the most important punchlines of Chapter 3 will be elevated to prominance in the EXECUTIVE summary in the Working Group 1 conclusions in the final AR4 report. For me, the most original and seminal conclusions in this Chapter 3 are in the substantial improvements in the descriptions and conclusions concerning the measurements of water vapour trends that agree impressively well with the projections from virtually all of the world's climate models. [Jerry Mahlman]	
3-60	A	0:0		Finally, I would like to assert that IPCC WG1 Chapter 3 needs to make a very forceful statement regarding the MEGA-IMPORTANT point concerning the continuing embarassingly poor, and declining, state of the world's tattered and beleagured Climate Monitoring System. Is not now the time to make a strongly accelerating case to address the compelling challlenges of human-induced climate warming in globally co-operative commitments to put together the World's First Real Climate Monitoring System? Clearly, their is a comparatively small global "cost" of this highly focussed, century-scale, global climate monitoring system to evaluate the strengths and weaknesses of human-induced climate warming theory and modelling. Obviously, WMO and the World Climate Research Programme have not come close to achieving this. Because the global warming signals are now inexorably emerging, it is now more important than ever for IPCC and the world's richest countries to stand together to commit to this internationally compelling need. What better place is there than its Policymakers Summary in its most compelling Chapter 3 punchline: The earth is warming, atmospheric water vapour amounts are increasing, humans are causing most of these changes, and these changes are quite	

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				consistent with the projections from the world's leading climate models. [Jerry Mahlman]	
3-61	A	0:0		It has been a pleasure, and a burden, to read and review this crucially important, but challenging IPCC WG1 Chapter 3 on Observatons, and the various attempts to synthesize them into a coherent and robustly sound observational assessment. In my opinion, the Chapter is roughly twice as long as it needs to be.. The "two different worlds" of sections 3.1-3.4, and of sections 3.5-3.8 remain jarring, and somewhat discouraging. I am not optimistic, but I still suggest that it would be a laudable achievement if these "two different worlds" could actually find some meaningful contact with each other. It would be very satisfying to see this being achieved, but I suspect that the sheer exhaustion factor will be sufficient to make it very difficult to merge this into a more cohesive document than that of this current Chapter 3 review draft. [Jerry Mahlman]	
3-62	A	0:0		I was impressed by the lack of typographical errors and incoherent statements. This is easier said than done in the microscopic print that is used in this "First Order Draft of [Jerry Mahlman]	
3-63	A	0:0		An acronym listing is especially needed for Chapter 3. [Steven Massie]	
3-64	A	0:0		Why some points of measurements (observation) far from urbanization such as Assekem in the Hoggar (Algerian Sahara) are not considered, to show the climatic change in temperature. [Amar Matari]	
3-65	A	0:0		In general for completeness there should be better descriptions of many of the observation types discussed in this report. For example HIRS does not directly measure UTH. [Mark McCarthy]	
3-66	A	0:0		In general, let me register a complaint that where a published paper apparently supports a position favoured by the Chapter lead authors, it is reported uncritically and given the most generous interpretation, with no discussion of critical perspectives on the same question; but where a published result runs against the position favoured by the lead authors it is either ignored altogether, or (like Kalnay and Cai) subjected to heavy disputation with favourable attention given to any critics. The overall impression I am left with is that the writers were overly alert for evidence backing up a prior position and are presenting a brief for "their side", rather than providing a balanced survey of the field. [Ross McKitrick]	
3-67	A	0:0		As the authors state in section 3.1 (Introduction), the chapter focuses almost only on the post-1979 time period. I agree that the most important results since the TAR concern this period, but I think that the more remarkable new results concerning previous years should	

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				also be summarized. In particular, I would suggest to highlight the efforts performed within some EU-funded projects (e.g. ALPCLIM/ALP-IMP, EMULATE, etc...) and other European initiatives (e.g. the ECA), that allowed to improve significantly the quality and the availability of secular records. Probably the authors do not give emphasis to such activities as global and hemispheric temperature reconstructions have not changed significantly since the TAR. However, this is not always true at regional level, where the availability of better data sometimes allowed to correct the previous temperature trend estimates and to extend the secular reconstructions to other variables. Some relevant references are: Böhm et al., 2001; Auer et al., 2005 (already included in the list of references) and Auer et al., 2006, that is the key reference publication for HISTALP, the new instrumental database in the Greater Alpine Region. REFERENCES: Böhm R, Auer I, Brunetti M, Maugeri M, Nanni T, Schöner W. 2001. Regional Temperature Variability in the European Alps: 1760-1998 from homogenised instrumental time series. Int. J. Climatol. 21, 1779-1801. Auer, I., et al., 2005: A new instrumental precipitation dataset in the greater alpine region for the period 1800-2002. Int. J. Climatol., 24, 139–166. Auer I., et al., 2006: HISTALP – Historical Instrumental climatological Surface Time Series of the Greater Alpine Region: 1760-2003. Submitted to Int. J. Climatol., Oct. 2005. [Teresa NANNI]	
3-68	A	0:0		Overall, this is an excellent chapter and provides a well-balanced view of the topic. Most of my comments are about matters of style, some about suggesting that some content would be more appropriate in other chapters. But it is a very good "first" draft. [Neville Nicholls]	
3-69	A	0:0		It is not an easy work to give a balanced assessment of recent research works in such a short period. The authors of this chapter have done a good work, and it should be greatly appreciated. Further improvement could be done, however, to make it much better and more acceptable for most of climate change researchers. I would suggest that the authors pay more attention to: a. publications not included in the reference list, especially those showing some facts and views different from those of authors; b. uncertainties in surface observation data sets; c. publications written in languages other than English; research results done by developing country scientists. I would also be happy to see a much larger number of reviewers in the expert reviewing process. [Guoyu REN]	
3-70	A	0:0		The treatment of the surface water balance, especially evaporation, has major inconsistencies throughout this chapter. For example, observations show; near constant relative humidity near the surface (p. 32, section 3.4.2). Precipitation onto land is in	

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				general increasing very slightly (section 3.3.2). The limited observations also show in general, increasing soil moisture (section 3.3.4). Pan evaporation is declining and this is a measure of potential evaporation (Box 3.1). Hence potential evaporation is declining. All of these changes paint a picture of the surface "wetting up". However, the overall thrust of the chapter is one of drying (section 3.3.4, p. 22 - p. 23, also see the executive summary (p. 4, lines 24-34). The basis for all these assertions appears to be the paper by Dai et al. 2004 that uses the PDSI to calculate trends in droughts. The calculations involve the use of a simple bucket model that is reasonable. The calculations also use the Thornthwaite method to estimate potential evaporation. This is UNREASONABLE. The Thornthwaite method assumes that potential evaporation changes because of changes in mean air temperature. This is WRONG and has been shown to be wrong on many previous occasions. The increase in drought described throughout the chapter is based on increasing potential evaporation calculated by the Thornthwaite method. Of course, this is in contradiction to the pan evaporation measurements which show a decrease. This question was recently addressed by Chen et al. 2005 (Comparison of the Thornthwaite method and pan data with the standard Penman-Monteith estimates of reference evapotranspiration in China. Climate Research 28: 123-132.). They showed that the Thornthwaite method gave increasing trends in potential evaporation because of increasing average air temperature. However, the FAO recommended Penman-Monteith method gives a declining trend in agreement with the pan measurements. If either the pan measurements, or Penman-Monteith calculations were used to estimate potential evaporation, then the potential evaporation would in general have declined over the period, and the PDSI would undoubtedly show a general reduction in drought - the opposite of the main conclusion of the chapter. This must BE FIXED. [Michael Roderick]	
3-71	A	0:0		Throughout this chapter there is confusion between evaporation and potential evaporation. This must be FIXED. For example, in the executive summary, p. 4, lines 14-22 we have increased potential evaporation because of warming. In the next paragraph, p. 4, lines 24-34, we have declining pan evaporation, and hence declining potential evaporation. This is the opposite of the previous paragraph. More generally, the latter paragraph (p. 4, lines 24-34) paints a picture of "wetting up" while the former paints the reverse picture, i.e., droughts and "drying out". The same problem occurs throughout the chapter. See comment 21. [Michael Roderick]	
3-72	A	0:0		A huge and relevant work has been performed to synthetize all observed atmospheric changes. [Paolo Michele Ruti]	

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3-73	A	0:0		Harmonization in writing styles is required across sections. The ES and 3.1 were clear, and section 3.2 follows the approach taken in the TAR. Sections 3.3 and 3.4 were much 'denser' to read than section 3.5 , 3.6, 3.7 and 3.8. I would recommend that the CLAs in particularly look towards homogenising the writing style throughout. [M James Salinger]	
3-74	A	0:0		1961-1990 is chosen as the 'normal' period with reference especially to temperature anomaly fields, and I know that this is an excellent period for which station numbers are high and coverage. IPCC authors and bureau may wish to consider moving to a more current period of climate as the reference period for AR5 (the IPCC assessment after this one). This would be more relevant particularly for applications as this will be the period of climate civil society is more adapted to. When it comes to adaptation, anomalies around a mean centred on the mid 1980s is better than the mid 1970s, especially in 2007. However, I know this is not a trivial task. [M James Salinger]	
3-80	A	0:0		A very sound, detailed, and instructive text where only a few shortcomings (major in section 3.8, otherwise minor) are addressed in the following, see specific comments. [Christian-D. Schoenwiese]	
3-81	A	0:0		Having reviewed the ZOD, I find the FOD to be a major improvement in several important ways. The figures are much improved and easier to interpet. There is more summary information at the end of sections that helps pull together the main findings. And the text is somewhat more uniform in style and tone throughout the chapter, although further improvements in this area could be made. [Dian Seidel]	
3-82	A	0:0		A few suggestions that I made regarding the ZOD were not implemented, yet I still feel obliged to make these points, so here they are. First, I think the notion of the inadequacy of most of the main observational datasets to address climate trends is pervasive in the text and therefore deserves to be brought forward in the Ex. Summary. Second, there remains a lot of "tutorial" material in the chapter that is nicely written and very informative but does not really represent advance in understanding since the TAR, which I thought was supposed to be the main thrust of the AR4. By including the background tutorials, the chapter is much longer than it would otherwise be. Finally, while the end-of-section summaries are helpful, the chapter could also benefit by the more frequent use of "topic sentences" at the start of sections or even paragraphs. A lot of sections contain series of sentences/statements that are not necessarily mutually supportive. Reading those is tough, since the main idea, if there is one, is hard to discern. A 'topic sentence' articulating the main idea of what is to follow would be a big help. [Dian Seidel]	

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3-83	A	0:0		A general comment on the figures that show time series. Many show data after having applied temporal filters, but there is a wide variety of filters used. It's not a major point, but wouldn't it be nicer to use the same filter for all the figures? Note that Figs 3.2.1, 3.2.7, 3.2.8, 3.6.8 use a 21 point binomial filter, Fig. 3.2.2 uses a 9 point binomial filter, Fig. 3.2.5 uses a 12 point filter, Fig. 3.6.3 uses a 7 point filter, and Fig. 3.7.4 uses a "low pass" filter. [Dian Seidel]	
3-84	A	0:0		In general, there are parts of this chapter that go on and on about results in the literature that may not be really useful or credible. In my view, the Assessment part of the IPCC AR4 process should be reaching decisions about which reported results are credible and which are not, then reporting a summary (not the details) of the credible results. I don't feel it should be necessary to cite every paper on every topic covered. Specific examples are noted in other comments. [Dian Seidel]	
3-85	A	0:0		I frequently found in this chapter that thepresentation downplayed or misrepresented evidence that did not fitwith the lead authors' theories as to what is happening in the world; that results of weak statistical significance or with only partialagreement among observational analyses were often restated later inthe chapter as fact without any caveats; and that inappropriate andunnecessary allusions were sometimes made to human causes when describing observed phenomena. Some instances of this are detailed among my comments below. As someone who basically agrees with the predominant picture, I am nonetheless concerned that such misrepresentations, though they may seem innocent, will draw calls of bias. Of course such accusations will be launched anyway from certain quarters, but text that will give them more sticking power should be avoided. [Steven Sherwood]	
3-86	A	0:0		Just a mnior style comment, but "firstly" and "secondly" are not real words; also, although it has seeped into wide (mis)use, I still cannot stand the use of "impact" as a verb so I cast my 2c to change all these to the perfectly suitable "affect." [Steven Sherwood]	
3-87	A	0:0		It is well known that there are tens of definitions of "drought" from different scientific points of view, though this chapter seems to refer the term to a limited meaning of soil moisture (or hydrological) drought. A brief explanation should be added on the definition somewhere in this chapter. [Masato Shinoda]	
3-88	A	0:0		Suggest replace acronym TAR by AR3. TAR will not generalize unambiguously to	

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				fourth, fifth, sixth or seventh reports. This document is named …AR4… . Comment probably applies to whole report, but I've read only Chapter 3. [Adrian Simmons]	
3-89	A	0:0		There are too many references to papers by Trenberth. No disrespect to the extensive contributions of Kevin is intended by this remark, but sometimes there are references to Trenberth publications when others would equally do, and it would certainly look better if this Chapter does not appear overly to report on the findings of one of the lead authors. A few examples are given below. [Adrian Simmons]	
3-90	A	0:0		Use of present tense for changes and trends that have been noted in past observations. Throughout the chapter, there are numerous instances of where the present tense is used to refer to trends or changes noted in past observations – for example, Question 3.2 how is precipitation changing? This is misleading since it gives the false impression that past trends will continue into the near future. Observations tell us about changes and trends in the PAST not the PRESENT or FUTURE. I strongly urge the authors to adopt the same approach as is widely used in macroeconomics, and use the past tense to refer to all changes and trends noted in past observations e.g. from 1970-1995 there was an increasing trend in NAO. To imply that past trends will continue into the present and future requires a model as well as observations. [David Stephenson]	
3-91	A	0:0		The authors have attempted to assess the statistical significance of linear trends by quoting approximate 95% confidence intervals (+/- 2 standard errors). [David Stephenson]	
3-92	A	0:0		Throughout the chapter, the authors tend to invoke changes in the ocean thermohaline circulation as evidence of and being caused by global warming. This evidence is far more tentative than the authors imply, and in many cases is inconsistent with what is written in chapter 6. The two chapters should be reconciled. Chapter 6 is a more careful and complete summary of ocean changes and should be used in chapter 3 as a reference. [LuAnne Thompson]	
3-93	A	0:0		There is no primer on how we have observed the climate system during the period of instrumental record and the problems that ensue. These are referred to in numerous places, but in such dribs and drabs that the story is not told coherently. A box describing the historical observing system including an illustration of postage stamps showing changes in in-situ station reporting density over time (surface and radiosonde) and a time-line of satellites (akin to plots provided by saki and colleagues at ECMWF) and then a discussion of the problems it represents from a climate monitoring perspective would be	

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				of enormous benefit and could be cross-referenced where necessary. This "assumed knowledge" is critical and it is important not to make the assumption that the multi-disciplinary audience will understand the historical measurement techniques and just how much these have changed over time. [Peter Thorne]	
3-94	A	0:0		Uncertainty is very poorly communicated throughout. There are no consistent defenitions of what is being discussed and there is no primer for the general audience.There is too much assumed knowledge. Requires a box defining the different sources of uncertainty: structural (methodological choices), parametric (applying these to finite imperfect data), and statistical (fitting of a straight line etc. to the resulting timeseries) - these are used in a pick and mix manner and all simply referred to as uncertainty in most cases. But they are very different entities. So I would urge a formal definition and then adoption of the terms formally defined when discussing uncertainties so that the readers can understand what is going on and what specific sources of uncertainty are being considered in each case. The CCSP appendix and a paper to appear in October edition of BAMS discussing the uncertainty issue, which has been edited out from my original contribution, should be referred to. [Peter Thorne]	
3-95	A	0:0		In many cases the current chapter gives the impression of deciding which dataset is best if several datasets are available, but in all cases this is a subjective decision, or at best (if I'm being generous) a semi-objective decision. Unless there is over-whelming community-wide agreement (which there isn't) or the dataset has been officially withdrawn by its originators (which they haven't) I am deeply deeply uncomfortable with such assertions being made. The chapter needs to be far less dismissive of structural uncertainty and to be more even-handed in its approach to a large number of the datasets being considered. Particularly in the realm of upper-air temperatures (see later comments), but also more generally. [Peter Thorne]	
3-96	A	0:0		The descriptions of trends and changes in regions sound very much like a shopping list, are very hard to read, and make much of the chapter virtually impenetrable. Much is text describing data in the figures, and as the figures exist why are they then being explained in such detail? Surely the figures speak for themselves? At a minimum the sentences need a lot of work to make these sections describing regional changes far more readable. An alternative, which I strongly recommend, is to tabulate changes in different variables by some pre-defined areas e.g NW Europe, such that variables are the columns and regions the rows. This allows a synthesis of all the information from the figures and all the different sections in one single place. And has the "bonus" of removing large sections of	

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				shopping list from the text. To a policy maker such a tabulated summary of changes by region in all the variables being discussed would be of enormous benefit as it avoids the need to go through every single section in huge detail. The text can then support the table. [Peter Thorne]	
3-97	A	0:0		There needs to be consistency in how the separate reanalyses are referred to. NRA, NCEP, NCEP/NCAR, NCEP/NCAR-2 - you can take your pick and find it somewhere in this chapter despite the fact that they are referring essentially to the same dataset. [Peter Thorne]	
3-98	A	0:0		There is ambiguity throughout the chapter as to the definition of the tropics - are they 20N to 20S, 30N to 30S or some hybrid? This doesn't make any sense to the reader if the definition is constantly chopping and changing. The region 20N to 20S is pretty much consistently within the Hadley Cell circulations whereas 30 to 30 includes ferrell cells so physically the former makes sense as it is describing a single physical unit of the atmosphere. [Peter Thorne]	
3-99	A	0:0		All timeseries figures: There are so many different smoothings applied that the reader will rightly be confused. They could rightly accuse different filters being used to "massage" the data for maximum impact / benefit. Hence this is unwise. Suggest that a standard filter methodology is applied to all timeseries plots and that at most the filter window width is changed. This standardisation will have many benefits and few drawbacks and make interpretation easier to the lay reader. [Peter Thorne]	
3-100	A	0:0		The way the message is presented is more assertive than in Chapter II. This is because the authors are different but also because there is perhaps more confidence in temperature observations than in radiative forcing estimates for instance. However, a qualitative check on the adjectives used (when communicating uncertainties for instance) could be useful for ensuring full consistency on the confidence level given throughout chapter II and III [Philippe Tulkens]	
3-101	A	0:0		I find that Canadian studies on ocean wave heights (see Comments #15-17) and Canadian surface climate (see Comments #14 and #24-25) are under-represented in this chapter. For example, only European studies (e.g., Alexandersson et al. 2000 and Alexander 2005b) are cited when talking about using station pressure data to study extra-tropical cyclone activity. The recent study by Wang et al. 2005b using station pressure tendencies (see Comment #6-7, #14 and #21 below) was not cited anywhere in this chapter, nor other Canadian studies on significant wave heights and frequency of freezing precipitation and blowing snow occurrence (see comments below). The PDF files of these papers are available via ftp to "ccrp.tor.ec.gc.ca" (or 199.212.19.40): Login as "anonymous"; use	

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				your email address as the password; enter "passive" (if not passive by default); change to "pub/Papers/Leona" directory by entering "cd pub/Papers/Leona". [Xiaolan L. WANG]	
3-102	A	0:0		The draft is written carefully and comprehensively. Even on controversial issues, the draft is written in a balanced way. [Koji Yamazaki]	
3-103	A	1:0	117:	Clearly, this is the best chapter of what I have read, for its comprehensiveness and the quality of information provided by the authors. There is one important point I wish to bring out> As they have shown the precipitation trend is negative from 1950 to now. This trend seems to be inconsitent with the global warming physics in models which would suggest that precip should have increased instead. There is also one disconnect between the rest of the chapter and the dimming chapter. The dimming chapter seems to conclude that beetween 1950 to 1990 the solar radiatie reduced globally by about -7 W m-2 or less(meaning more dimming). How did the planet not cool significantly in the face of such large reductions? A similar decrease due to volcanic eruptions over a shirte r period leads to cooling as lareg as -0.5C!!. I refer to Ramanathan et al (Science 2001) which shows how to avoid this conflict. [Veerabhadran Ramanathan]	
3-104	A	1:0	117:	There are several issues or inconsistencies in the chapter that needs to be addressed: 1) The Chapter explains away the major land drying from 1950 to 2004, as due to the negative correlation between land warming and land precip. This may apply if the land warms in isolation, but certainly can not be the case when the land warming is accompanied by ocean warming. WE can not ignore the fact that the entire NH ocean and land has warmed significantly and this acrording to GCM -global warming studies should have led to increase in precip. A more detalied discussion is needed. For example, I refer to Ramanathan et al (Science, Vol 294, 2001) which suggest that large dimming by absoebing aerosols can account for the decrease in precip in spite of the global warming. 2) Why is the NH warming more than SH warming? 3) The summary claims consitency between TOA flux anomalies and observed cloud changes. However, in the text we see discussion of how satellite cloud and radiation flux products are riddled with errors. 4) The dimming section suggests that some of the large decreases may be due to clouds; if clouds are blamed for dmming, you need to acknoeledge that the surface dimmaing will translate into a large negative TOA forcing making it nearly impossible to account for surface warming or ocean heat capacity increase. 5) I think it is important to show the temp trends as a function of season where he aerosol and GHGs signal are more	

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				distinguishable. For example, as shown ny Krishnan and Ramanathan (GRL, Vol 29, P. 54-1, 2002), the aerosol cooling effect clearly shows up during the dry season temps(October to May) when aerosol loading peaks, whereas the GHS warming continues unabated during the wet season. In the US for example, the aerosol loading peaks in summer.. [Veerabhadran Ramanathan]	
3-105	A	1:9	1:17	The list of author should be given with the full name of the people, as in Chapter 1 [Philippe Tulkens]	
3-106	A	1:16	1:16	Vanity comment: my middle initial is W, not D. [Peter Thorne]	
3-107	A	1:30	10:13	Besides the global surface record being inadequately corrected for urbanisation you make no attempt to correct it for known natural phenomena such as volcanos and El Niño. Jones used to deduct the supposed influence of El Niño at one time. Whatever happened to it? There has been a claimed success for prediction of effects of Pinatubo on temperature, so there should be no problem in subtracting it. What remains would be a more likely possible influence of human activity , although there could still be unresolved solar or other influences. The trouble is that removing the 1991 Pinatubo and the 1998 El Niño would reduce the slope of all records and it would reduce the MSU record more than the surface record, thus opening up a discrepoancy which you thought you had solved. A record that has been corrected for known natural events would provide a better target to be simulated by the models. [Vincent Gray]	
3-108	A	1:30		Replace “Surface” by “Global” [Vincent Gray]	
3-109	A	1:38		"3.3.4 Changes in Soil Moisture, Drought, Runoff and River Discharge": a whole sub-chapter 3.3.4 should be devoted to drought (with 3.3.5 = soil moisture, runoff and river discharge and 3.3.6 = Consistency and Relationships between Temperature and Precipitation). [Martine Rebetez]	
3-110	A	3:0		Create headings for each major variable like "Changes in Temperature and related variables" or "Changes in Atmospheric Circulation" with subheadings where clarity is further enhanced. [Chris Folland]	
3-111	A	3:0		Consider an Overview - a summary of the Executive Summary. This was effective in the TAR. [Chris Folland]	
3-112	A	3:0		Lines 27-31 contain an indigestible long sentence.	

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				[Chris Folland]	
3-113	A	3:0		Each bullet should stand alone. Thus DTR on line 36 should be spelt out as should all other acronyms, except where repeated within a bullet. [Chris Folland]	
3-114	A	3:1	5:22	Executive Summary. Suggest break up each finding, or closely related findings, into separate paragraphs headed by a bullet point, generally shorter than current paragraphs [Chris Folland]	
3-115	A	3:1	5:	I found the EXECUTIVE SUMMARY to be concise and clear, with appropriate highlighting of the most important punchlines in the Chapter 3 text. I was pleased by the rather robust conclusions concerning observed trends of DTR that are in rather good agreement with a-priori expectations from theory-based evaluations. [Jerry Mahlman]	
3-116	A	3:1		Executive Summary. The following two comments do not appear to be consistent; widespread drought (measured by PDSI, that is, a sort of soil moisture index) (p4, l14) vs. enhanced soil wetness (p4, l31). [Masato Shinoda]	
3-117	A	3:1		I found this exec summary to be heavily biased towards ENSO. There are dozens of climate indices and some (e.g. the North Atlantic Oscillation) are far more relevant to particular phenomena then ENSO. I feel this repeated and unbalanced representation of ENSO is dangerous as it is both misleading and can be easily mis-used by the media and even policy makers. [Steven Siems]	
3-118	A	3:3	3:16	This paragraph implies greater confidence in the trend observations than seems warranted, for example, by Table 3.2. [Marcia Baker]	
3-119	A	3:3	3:6	It is not clear from the text if the GISS data has its own independent SST analyses from other two datasets to support such statement. [Qiang Fu]	
3-120	A	3:3	3:4	… The linear trends are 0.062, 0.056, 0.064, and 0.058 K decade-1, for estimates compiled by CRU/UKMO, NCDC, Russian SHI, and GISS, respectively,…. [Pavel Groisman]	
3-121	A	3:3	3:3	Global-mean temperatures averaged over land and ocean surfaces: Is there any description in the text how to calculate these mean temperatures from the observation data? This kind of question is very simple, but annoying readers. [Shigeki Kobayashi]	
3-122	A	3:3	3:8	The differences in how trends in temperature are being expressed in this assessment	

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				relative to the TAR is a key issue that will attract a great deal of attention. It would probably be helpful to many readers if the authors would state in a few lines in this executive summary how this approach differs from the TAR, and why it is being done this way. [Susan Solomon]	
3-123	A	3:4		Replace “independently” by “partially”. Most of the data outside the USA cannot be satisfactorily “adjusted” for “homogeneity” (Peterson et al 1998, Peterson 2003, Hansen et al 2001) [Vincent Gray]	
3-124	A	3:4		Replace “consistent” with “intermittent” [Vincent Gray]	
3-125	A	3:4		Insert “where possible” after “adjusted” [Vincent Gray]	
3-126	A	3:4		Replace “linear” by “linearized” [Vincent Gray]	
3-127	A	3:5	3:8	Need a statement that says total global surface warming over 1901-2004 has been 0.6+-0.2C or whatever the values are. This form of statement is needed for the Policymakers Summary. Somewhere near here it needs to be concluded that global warming since the beginning of the twentieth century is "virtually certain". [Chris Folland]	
3-128	A	3:5		the linear trendis 0,056 for NCDC but in the table 3.3 page 13 we read 0,057 [Matari Amar]	
3-129	A	3:5		This Chapter gives information about many different records of global temperature change. Why have you chosen this particular aspect of the surface record for undue publicity? Is it because it is the largest figure you could find, so as to bolster the case for greenhouse warming, and as a result induce Chapter 8 to imply that it is the only record that needs to be considered? [Vincent Gray]	
3-130	A	3:5		the linear tend is 0.056 for NCDC but in the table 3.3 page 13 we have 0.057. [Amar Matari]	
3-131	A	3:7	3:7	"it is very likely that a warming of 0.6K occurred over the 20th century". If 'very likely' is to be used here, then I would suggest that the 5-95% confidence range be given on this number. Otherwise it is not clear what exactly is meant - is it that the warming is 'very likely greater than 0.6K', 'very likely 0.6K to within 1 d.p.', or something else? [Nathan Gillett]	
3-132	A	3:7	3:7	I don’t like the clause "..it is very likely that a warming of 0.6K occurred over the 20th	

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				century". I cant see how we can attach a probability to a specific warming. I would think something like "…it is virtually certain that a warming of AT LEAST 0.55K occurred over the 20th century". Note that using the term "at least 0.55K" allows us to increase the likelihood to virtually certain. [Neville Nicholls]	
3-133	A	3:7	3:8	Excellent idea to bring attention to accelerated warming and the non-linearity. [Neville Nicholls]	
3-134	A	3:7	3:8	Two values are given for the estimate of global mean surface temperature increase (respectively 0.6 and 0.75 K). The rationale for giving two different estimates makes sense to specialists. However, in order to clarify the communication of the results, can't the IPCC agree on one figure (with an error bar) that would be considered most representative? [Philippe Tulkens]	
3-135	A	3:8	3:8	This sentence is not clear. Is 1860-1900 a 40 years reference period? [Franco Desiato]	
3-136	A	3:8	3:8	may be more informative to say that an additional warming of about 0.15 K has been observed between 2000 and 2004 [Bernard Seguin]	
3-137	A	3:8		make un choice between the period 1860-1900 or 1861- See line 15 and table 3.2 [Matari Amar]	
3-138	A	3:8		make a choice between the period 1860-1900 or 1861 (see line 15 and table 3.2) [Amar Matari]	
3-139	A	3:8		The 0.75K should read 0.75K/decade. [David Stephenson]	
3-140	A	3:9	3:10	This statement is at odds with Christy et al 2001 who found "A global surface temperature dataset which uses only near-surface air temperature over both land and ocean indicates less warming since 1979 than those using SSTs over the oceans" [Vincent Gray]	
3-141	A	3:9		Insert at the beginning "About 0.4K of this rise took place before 1940, and all of the remaining 0.4K occurred after 1980. This figure is subject to bias from socioeconomic factors, as revealed by the statistical studies of McKitrick and Michaels (2004), and another upwards bias has resukted from the lower temperatures at the beginning of this sequence from the volcanos El Chichon (1983) and Pinatubo (1991), and the raised tempeature at the end of the sequence from the El Niño event of 1998 [Vincent Gray]	
3-142	A	3:10	3:10	Please add (to reduce the US-Chinese bias): Well proved evidence from Central Europe	

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				(Auer et al., 2001, Auer et al., 2005b) tells that even the most contradictory thinkable respective station types (rural high elevation summit sites vs. low elevation urban sites) show no urban bias for the last 100 to 150 years after homogenisation. [Reinhard Böhm]	
3-143	A	3:10	3:22	Should not mention be made of the active 2005 hurricane season ? [Robert KANDEL]	
3-144	A	3:10		correct: "nightime" to "nighttime" [Hartmut Grassl]	
3-145	A	3:11	3:12	0.5 K is for "mid 1970s -2004", 1.15-1.18 is for 1979-2004. The warming for 1979-2004 is closer to 0,4K than 0,5K. [Jean-Marc Moisselin]	
3-146	A	3:11		it is preferable to mention late 1970 instead of mid 1970s1 [Matari Amar]	
3-147	A	3:11		Insert at the beginning “After a period of falling temperature from 1941 to 1980 of about 0.05K”, [Vincent Gray]	
3-148	A	3:11		Replace “Rates of temperature rise are grester “ with “Temperatures began to rise again” [Vincent Gray]	
3-149	A	3:11		it is preferable to mention late 1970s instead of mid 1970s [Amar Matari]	
3-150	A	3:12	3:12	delete the following part of a sentence: "for a total warming of 0.5 K," (it makes no sence here) [Reinhard Böhm]	
3-151	A	3:12	3:13	Can we trust the apparent weaker warming over oceans? I thought the Hadley Centre suggests that part of this weaker warming may reflect a bias from increased use of buoys? [Neville Nicholls]	
3-152	A	3:13	3:13	in" should be "during [Patrick Minnis]	
3-153	A	3:13		Delete "northern" as sentence ends "in the NH" [Adrian Simmons]	
3-154	A	3:14	3:16	2005 may have equalled or exceeded 1998 as the warmest on record. Should update as needed. [Henry Diaz]	
3-155	A	3:14	3:16	NASA GISS has webposted results indicating that 2005 may well turn out to be globally hotter than 1998. [Robert KANDEL]	

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3-156	A	3:14		Insert after “year” “for this particular observation set” [Vincent Gray]	
3-157	A	3:15	3:15	Change El Niño for "El Niño event" [Enric Aguilar]	
3-158	A	3:16	34:23	3-34 lines 16-23 as well as 3-34 lines 47-49 I therefore recommend that the trend statement be removed or heavily clarified. On a more positive note, I believe the scientific literature since the TAR has shown a new capability to measure and analyze interannual variations of global water vapor. Furthermore, our own analyses of the NVAP records published in the 2 BAMS articles also show a near constant global relative humidity with, however, some interesting regional-temporal exceptions. [Thomas Vonder Haar]	
3-159	A	3:16		read among the ten warmest years, the exception is 1966 [Matari Amar]	
3-160	A	3:16		Add at end “for this particlar observation series” [Vincent Gray]	
3-161	A	3:16		among the ten warmest years, the exception is 1996 [Amar Matari]	
3-162	A	3:17		Insert a paragraph giving the results of reanalysis of surface measurements, including the finding of Kalnay and Cai 2003 that urbanisation and land use changes are a significant source of bias. [Vincent Gray]	
3-163	A	3:17		Insert a paragraph summarising the results of temperature changes of proxy measurements since 1900; they show equivocal evidence of warming (see Figures 6.8 and 6.9) [Vincent Gray]	
3-164	A	3:17		Insert lines 33-37, as amended [Vincent Gray]	
3-165	A	3:18		Replace “consistent with” by “related to” [Vincent Gray]	
3-166	A	3:19	3:19	perhaps "daily" should be inserted before "warm extremes" [Franco Desiato]	
3-167	A	3:20	3:20	please define "cold nights". Is it in relative (percentile, …?) or absolute (min below any absolute threshold,…?) terms? [Reinhard Böhm]	
3-168	A	3:20	3:20	Define "cold nights".	

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				[Qiang Fu]	
3-169	A	3:20	3:24	two periods : 1951-2003 and 1950-2004 [Jean-Marc Moisselin]	
3-170	A	3:20	3:21	These two sentences are curiously written. Very precise percentages are quoted for the changes in the numbers of "cold" nights and "warm" nights, but these are meaningless unless it is defined what is meant by "cold" and "warm". [Adrian Simmons]	
3-171	A	3:20		the limits of temperature for cold night and warm night must be specified [Matari Amar]	
3-172	A	3:20		"cold night" is rather vague given the detailed %changes listed, give index? [Gabriele Hegerl]	
3-173	A	3:20		the limits of temperature for cold night and warm night must be specified [Amar Matari]	
3-174	A	3:21	3:24	The first sentence conflicts with the second: (the first saying that the DTR-trend has continued, the second saying it has stopped in the 1979-2004 period) [Reinhard Böhm]	
3-175	A	3:21	3:23	two expressions for the same thing ?"of the land regions studied", "land surface where data where available" [Jean-Marc Moisselin]	
3-176	A	3:21	3:24	a small contradiction between the greater increase of nightime compared to daytime and the zero change of DTR between 1979 and 2004 [Bernard Seguin]	
3-177	A	3:21	3:24	The sentence ending on line 22 is contradicted by the second clause of the following sentence. [Steven Sherwood]	
3-178	A	3:24		calculate the DTR between 1950-1978 and 1979-2004 [Matari Amar]	
3-179	A	3:24		calculate the DTR between 1950-1978 and 1979-2004 [Amar Matari]	
3-180	A	3:24	:27	There is an irritating use of record breaking statistics in this chapter which are at best anecdotal and at worst misleading. As instrumental records get longer, there will be an increasing number of record breakers even in the absence of climate change and so the occurrence of a record breaker does not help provide evidence in favour of climate change. Sustained trends in historical observations provide much stronger scientific evidence. The example in lines 24-27 of the abstract also has several other problems: it fails to state clearly which temperature series is being discussed (where was it? if it was	

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				located in the centre of the 2003 heat wave then this invalidates the statistical significance since the site was chosen AFTER the heat wave event), it fails to explain why the previous warmest was so far back in 1807 in the pre-industrial era, it fails to state clearly which temperature variable was used (2m air temperature?) and which JJA statistic was used. The other record breaking examples scattered throughout the chapter have similar problems and make the chapter into a more tabloid style sensational read rather than a serious assessment of scientific facts. [David Stephenson]	
3-181	A	3:25	3:25	Suggest adding "(JJA)" after "in the summer" and change "The summer (JJA)" to "This extreme". [Qiang Fu]	
3-182	A	3:25	3:25	. " The summer (JJA) (…)". The year 2003 should be specified. [Philippe Tulkens]	
3-183	A	3:25	4:27	The trend in total column water vapor mentioned here and on page 3-4 is very preliminary and very uncertain. Other global water vapor data sets from satellite, as reported by Randel, et. al. (BAMS, 1996) and Amenu and Kumar (BAMS, 2005), that contain information over both oceans and land should show that a trend in this parameter is very uncertain due to large estimated errors. Additionally, the NVAP dataset has never undergone a reanalysis to sharpen its trend-detecting capability. Indeed, while application of the Null Hypothesis to the currently available global satellite record can not rule out the possibility of a trend, the existence of several significant physical, interannual events in the relatively short global data record makes any statement of a trend very uncertain and speculative. Examples are the low values in 1988-89, Pinatubo in 1991, and the large ENSO in 1997-98. [Thomas Vonder Haar]	
3-184	A	3:27	3:31	Too long sentence! I suggest to re-write it [Javier Martin-Vide]	
3-185	A	3:27	3:31	This sentence is too complex and should be rewritten. By the time you get to the end of the sentence, you've forgotten the main message, that SSTs have been on the rise. [Dian Seidel]	
3-186	A	3:28	3:28	The meaning of "thermohaline signatures" here is not clear. [Qiang Fu]	
3-187	A	3:28	3:28	"thermohaline signatures". The expression should be explained, this is a specialized term [Philippe Tulkens]	
3-188	A	3:28	4:28	remove the comments about the thermohaline circulation [LuAnne Thompson]	

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3-189	A	3:33	3:37	The conclusion about negligible effect of urbanization on contental scale temperature averages are based on studies mostly done in the USA and one from China. Nothing is mentioned (in the chapter) about the urbanization effect on climate of other highly urbanized regions such as Europe or South East Asia. Therefore I suggest that this statement should say that urbanization has negligible effects on continental scale temperatures of the USA and China. However little is known about the urbanization effect on temperatures of other highly urbanized regions. [Galina Churkina]	
3-190	A	3:33	3:35	This statement is not true. McKitrick and Michaels 2004 Climate Research 26 150-173 have shown that the record has a significant influence of several socio economic factors, and suffers from serious defects. Also, the homogeneity corrections available in the USA cannot be applied to most other records.(Peterson et al 1998, Peterson 2003, Hansen et al 2001 [Vincent Gray]	
3-191	A	3:33	3:37	Here also include reference to a recent paper on station trends in solar radiation. Alpert, P., P. Kischa, Y.J. Kaufman, and R. Schwarzbard: Global dimming or local dimming?: Effect of urbanization on sunlight availability. Geophysical Research Letters, (32) L17802, 2005. [Michael Hobbins]	
3-193	A	3:33	3:37	This summary refers to discussion on pages 9-10 that need to be revised (see below); hence the summary needs revision mutatis mutandis. [Ross McKitrick]	
3-194	A	3:34	3:35	Delete from “as far as” on line 34 to “accounted for” on line 35 [Vincent Gray]	
3-195	A	3:34		Replace “negligible” by “significant” [Vincent Gray]	
3-196	A	3:35	3:35	Change "very real" to "real" (something is either real or not) [FILIPPO GIORGI]	
3-197	A	3:35	3:35	Could this sentence be completed thus "…acounted for IN THE DEVELOPMENT OF GLOBAL DATA SETS". [Neville Nicholls]	
3-198	A	3:35		urbanization does not affect the trend in SST. A point worth including? [Rasmus E. Benestad]	
3-199	A	3:35		"accounted for" - list briefly how, since this tends to be such a major concern in discussions [Gabriele Hegerl]	

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3-200	A	3:36	37:37	Chapter 3 Page 3-37 Lines 36-37 Page 3-38 Lines 36-37 Some of the questions noted in this section about the ISCCP global cloud results, as noted in lines 30 and 31, have recently been quantified in the Campbell and Vonder Haar 2005 paper. We will provide a figure extracted from the publication that has been accepted by GRL. Essentially, the analyses of the ISCCP cloud record for the effects of periods of satellite coverage and the resulting cloud view angles built into the ISCCP record has shown that a major portion of the downward trend reported by other scientists is an artifact of the observational situation. Therefore, the difficulty to explain the downward trend in global cloud cover is actually less than initially believed. I believe this puts the information about satellite cloud observations and the surface observations on a more comparable level. However, the inherent difference between those two data sets has been known for decades and can not be expected to be easily reconciled. [Thomas Vonder Haar]	
3-201	A	3:37		I suggest replacing "alleviation of" by "lower". [Adrian Simmons]	
3-202	A	3:39	3:51	This paragraph has an inconsistent level of detail (too great) compared with the rest of the summary [Steven Siems]	
3-203	A	3:39	3:41	This sentence is mis-leading. Fitting linear trends to the data does yield similar rates of warming, but the radiosonde warming occurs primarily as a result of a step-change in the late 1970's associated with the regime shift, whereas surface data show a more exponential increase with time. The sentence implies similarity of timeseries behaviour which is only supported by use of a mis-leadingly simple linear trend diagnostic. At a minimum a caveat needs to be appended to this affect. [Peter Thorne]	
3-204	A	3:39		Replace “similar” with ” somewhat lower”. (see Figure 3.4.2) [Vincent Gray]	
3-205	A	3:39		Again , it was discouraging that the disrepair of the global radiosonde network continues, to the detriment of weather and climate science. Surely, this unpalatable fact is an international embarassent for this, and for other important measurement systems, used to "monitor" climate variability, warming, and moistening. [Jerry Mahlman]	
3-206	A	3:41	3:45	Sentence reads badly - at minimum requires comma after '1979' on line 43.	

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				[John Caesar]	
3-207	A	3:41	3:51	The deflation of the earlier controversy could be put even more strongly. [Robert KANDEL]	
3-208	A	3:41		Replace “increasing” with “some” [Vincent Gray]	
3-209	A	3:41		Add after “tropics” Radiosonde results show a high level of agreement with MSU satellite results, but not with the surface record (see Figure 3.4.2) [Vincent Gray]	
3-210	A	3:41		You need a new paragraph to comment on MSU results. Start a new paragraph after “While” [Vincent Gray]	
3-211	A	3:41		Insert before “While” in a new paragraph: “The discrepancy between the results of the MSU satellites and the surface record has been reduced by further attention to satellite errors, but there is still a difference between them. The linear trend of the MSU is now 0.123 K per decade for 1979-2004 compared with 0.351K for the surface record, but the discrepancy may be greater if account is taken of the very large effect of the 1998 El Niño event on MSU” [Vincent Gray]	
3-212	A	3:43		Replace “likely” by “may” [Vincent Gray]	
3-213	A	3:44		Delete “substantially” [Vincent Gray]	
3-214	A	3:45	3:51	What has happened to the UAH-MSU in this statement? There does not seem to be sufficient justification for this passage from the observations. Furthermore, MSU has poor vertical resolution so where is the observational evidence for warming that increases with altitude, the subsequent text is not convincing on that particular point. [Mark McCarthy]	
3-215	A	3:45	3:48	The ERA-40 analysis exhibits vertical trend structure which is very likely the result of residual errors. When averaged using a static weighting function and a linear trend calculated these factors happen to cancel. However, use of ERA-40 as a supporting statement is highly highly dubious for this reason. Either a caveat needs to be placed, or, preferably, this text removed as it is disingeneous. I note that there was unanimous agreement amongst the CCSP authors that reanalyses were inadequate for upper-air trend characterisation. It is untenable for the "common authors" to be associated with both viewpoints. [Peter Thorne]	

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3-216	A	3:46	3:48	There seems to be a major disconnect here. UAH and radiosonde trends since 1979 are not consistent with the surface as shown in Fig. 3.4.3. Too, Fig. 3.4.3 shows ERA-40 T2LT and T2 agreeing with radiosonde and UAH satellite data, and is less positive than the surface. When proper significance testing is done (see below) these several trends are quite different from say RSS trends. Yet, here in the ES the conclusion seems to be that satellite and ERA-40 supports the trend of the surface – which they do not as they are cooler than the surface in tropics and globe. [John R Christy]	
3-217	A	3:50	3:50	Remove "lower". [Qiang Fu]	
3-218	A	3:50	3:51	The judgment that there is an increased rate of warming with altitude in the tropics does not appear to be a direct conclusion based on observations. Observation-based records fall on both sides of this question as is obvious in the previous sentence. It is models that only show increased warming rate with altitude. Suggest that it is made clear that differences between observation-based records prevent drawing this conclusion by themselves. Consideration of model results should be referenced forward to the appropriate chapter. [Haroon Kheshgi]	
3-219	A	3:50	3:51	It is not likely that there is increased warming from the surface, and especially in the tropics. That is too strong. The available evidence at the time of the TAR excluded this possibility, but new analyses suggest it is possible. That is a whole different ball game to the statement given here at present. Suggested alternative: "At the time of the TAR available estimates precluded warming aloft greater than that at the surface, particularly in the tropics. New research, leading to a number of new datasets, means that we can no longer state the relative sign of changes at the surface to those aloft." [Peter Thorne]	
3-220	A	3:50		Insert after “temperatures”. “ However, the MSU records are distorted by a very large effect from the 1998 El Niño, without which they would show less warming than the surface [Vincent Gray]	
3-221	A	3:52	3:52	global" is most often used to mean "global-mean", which is not meant here - I suggest "the global distribution of" or "the global field of [William Ingram]	
3-222	A	3:53	3:54	This is still wrong. Radiosondes show stratospheric cooling of ~0.6-0.8 K/decade since 1979. NRA also shows a larger trend. The bias estimates in Randel and Wu and Sherwood et al. are not the same as an "adjusted radiosonde" record. No one knows what a truly homogeneous sonde record would show. Why stretch the facts to create more	

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				agreement than there really is? [Melissa Free]	
3-223	A	3:53	3:54	This sentence is incorrect. Adjusted radiosondes still show a cooling rate since 1979 in the Tropics that is about double that shown by the satellites. In global means, this difference decreases, which is a key indicator that there are problems in the Tropical radiosondes. Reanalyses show a wide range of trends that are inconsistent with observations, and are not really relevant if important radiative forcing due to ozone changes were not included. [Steven Sherwood]	
3-224	A	3:53	3:54	This statement is plain wrong as evidenced in Figure 3.4.3 (which I will return to later). Uncertainty range in stratospheric temperatures is anywhere between -0.3K/decade (ERA-40) and -0.8K/decade (radiosondes) - that excludes the statistical (line-fit) uncertainty - it is simply the structural uncertainty. This text needs changing. [Peter Thorne]	
3-225	A	3:53	4:2	The first sentence of this paragraph says that all observations agree within 0.1K/decade. Then the radiosondes are criticised for possibly over-estimating stratospheric cooling. So is there good agreement or not? [Mark McCarthy]	
3-226	A	4:1	4:2	Again , it was discouraging that the disrepair of the global radiosonde network continues, to the detriment of weather and climate science. Surely, this unpalatable fact is an international embarassent for this, and for other important measurement systems, used to "monitor" climate variability, warming, and moistening. [Jerry Mahlman]	
3-227	A	4:1		Replace "sondes not yet accounted for" by "sonde instrumentation or real-time processing not accounted for by adjustments in the climate data records". Or reformulate the sentence in another way. It reads poorly as it is. [Adrian Simmons]	
3-228	A	4:4	4:5	The sentence relating precipitation, temperature, and streamflow is unclear. The confusion has to do with the idea of consistent changes. Does that mean temperature and precip both increasing or decreasing together? [Dian Seidel]	
3-229	A	4:5		This is a rather meaningless statement unless you define what you mean by “are consistent with measured changes in the streamflow”. You need to be a lot more precise about what you mean here – for example, by streamflow, do you mean geostrophic winds based on historical SLP observations. Consistent is a dangerous word to use throughout this chapter because it means different things to different readers – I would be tempted to	

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				avoid it wherever possible. [David Stephenson]	
3-230	A	4:6	4:7	Can you estimate the likelihood of an increase in global precip? "likely"? [Neville Nicholls]	
3-231	A	4:6	4:7	It would be helpful to express the rainfall in a unit that non-experts may better understand in an executive summary, such as percentage change. [Susan Solomon]	
3-232	A	4:6		has increased sighificatively over,,, [Matari Amar]	
3-233	A	4:6		changes in precipitation: % to me is easier to put into context than mm [Gabriele Hegerl]	
3-234	A	4:6		has increased significatively over .. [Amar Matari]	
3-235	A	4:7	4:10	Sometimes you are categorical about observed changes (eg "observed significant increasing amounts of water vapour"; in other cases you estimate a likelihod ("it is deemed likely that there are increases in the numbers of heavy precipitaton events..". Is there a logic behind this difference in treatment, or is it just due the problems of writing in a way that reads well? If the latter, a table to show the likelihood of all the observed changes might be sufficient, and you could then exclude all estimates of likelihood from the text of the summary. [Neville Nicholls]	
3-236	A	4:7	4:7	The words “per century” are not necessary [Philippe Tulkens]	
3-237	A	4:8	4:8	"(…) observed significant increasing amounts of water vapor in the atmosphere". Is the adjective "significant" consistent with the findings of Chapter 2 section 2.3.8? From my reading it is not very clear whether it is significant or not. Maybe the observed water vapor content has increased but its radiative impact is not significant (?). The consistency between the two sections should be checked. [Philippe Tulkens]	
3-238	A	4:9	4:9	seems that "numbers" should be "number', "from"mshould be "within" or "over" or "in" [Patrick Minnis]	
3-239	A	4:10	3:12	sentence about rare precipitations events not for executive summary because conclusions are not straight enough [Jean-Marc Moisselin]	
3-240	A	4:14	4:34	There appears to be some degree of inconsistency in discussion of evaporation variables.In lines 14-22 the statements imply decreases in R and increases in PE, yet in	

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				the following paragraph the statement is that PAN-E decresed. Reasons are given for possible discrepancies, but shoring up of these two paragraphs is needed. [Henry Diaz]	
3-241	A	4:15	4:34	See comment 21. [Michael Roderick]	
3-242	A	4:17	4:18	appears to also be a factor" should be "also appear to be factors [Patrick Minnis]	
3-243	A	4:17		Replace "appears" by "appear" [Adrian Simmons]	
3-244	A	4:18	4:19	Is it really possible to establish direct links of isolated extreme events to global warming? [Rasmus E. Benestad]	
3-245	A	4:18	4:22	"[i]ncreased temperatures and associated increased potential evapotranspiration that have enhanced evaporation and drying..." the causality picture and its implications for drought are more complicated than this simplistic sentence suggests. Constant RH (observations supporting which are claimed in this chapter) means that the vapour content of the air is increasing along with Temperature. So is evaporation (and what sort of evaporation is "evaporation"?) being driven up? The regional complementarity between ETa and ETp indicates that, the radiative dynamic assumed constant, increasing ETp means decreased ETa [*Bouchet, R. J., 1963: Évapotranspiration réelle et potentielle, signification climatique. International Association Scientific Hydrology, Proceedings, Berkeley, California, USA, Symp., Publ. No. 62: 134–142.; *Morton, F. I., 1983: Operational estimates of areal evapotranspiration and their significance to the science and practice of hydrology. Journal of Hydrology, 66: 1–76.; *Brutsaert, W., and H. Stricker, 1979: An advection-aridity approach to estimate actual regional evapotranspiration. Water Resources Research, 15(2): 443–450; *Hobbins, M. T., J. A. Ramírez, T. C. Brown, and L. H. J. M. Claessens, 2001a: The complementary relationship in estimation of regional evapotranspiration: the CRAE and Advection-Aridity models. Water Resources Research, 37(5): 1367–1388; *Hobbins, M. T., J. A. Ramírez, and T. C. Brown, 2001b: The complementary relationship in estimation of regional evapotranspiration: an enhanced Advection-Aridity model. Water Resources Research, 37(5): 1389–1404; *Hobbins et al., 2004. - the list of citations available on the complementary relationship is almost endless, and in the case of this chapter almost completely ignored]. [Michael Hobbins]	
3-247	A	4:18	4:19	The text (3-74) cites the well-known relationship between maximum temperature anomaly and rainfall deficiency over Australia. The evidence does not support the strength of the statement as it stands. [WILLIAM KININMONTH]	

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3-248	A	4:18		I do not like the statement "...have been inferred... By whom? This is really a loaded statement, and probably cannot be well supported. [Steven Sherwood]	
3-249	A	4:20	4:20	Specify "actual evapotranspiration" not just "evaporation." Also, ETp cannot enhance ETa: it is a result of ETa, not a driver. This is an example of note (ii) above (in #3). [Michael Hobbins]	
3-251	A	4:20	4:22	awkward sentence, perhaps, something like " Decreased precipitation and increased temperatures that enhance evaporation and drying are important factors have contrinuted to a rise in the number of region s suffering drought, as measured…" [Patrick Minnis]	
3-252	A	4:22	4:22	"Increased Tair and associated increased ETp that have enhanced evaporation and drying": see notes (ii) and (iv) above. Here we're missing the complementarity of evaporation (ETa is meant, I think) and ETp, and misdiagnosing the effects of Tair on ETp (by using the Thornthwaite expression for ETp). [Michael Hobbins]	
3-254	A	4:26		The time period cited here seems to be inconsistent with the chapter (see page 34, line 18). [Martin Manning]	
3-255	A	4:27	4:27	(a) If RH has remained constant, why is ETp increasing ETa (as claimed earlier)? (b) I don't buy that RH has remained "fairly constant". First, I wonder what a globally averaged RH tells us, as RH is such a local driver. Which is to say that hydrologic fluxes, particularly ETa, respond to variations of RH that are local in scale. But to changes in solar radiation that are local in scale but that are themselves driven top-down by regional variations. Second, you don't have to constrain your spatial scale much to see large variations: spatially averaging over the entire conterminous US, Hobbins et al., (2004) showed a decrease in VPD of 10.1% of the mean (i.e., increasing humidity) during the period 1953-1994. [Michael Hobbins]	
3-257	A	4:27	4:28	Similar trends have not been 'detected' in the upper troposphere. There are no reliable long-term records of specific humidity in the upper troposphere. There is some evidence that global mean relative humidity in the upper troposphere has no significant trend and any statement regarding trends in specific humidity are conjecture based upon this and temperature trends and therefore carries considerable uncertainty. [Mark McCarthy]	
3-258	A	4:28	4:30	Epan decreases are also due to variations in aerodynamic forcing, i.e., RH, or VPD, which have been shown to exist (Hobbins et al., 2004). [Michael Hobbins]	

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3-260	A	4:28	4:29	continental-scale estimates of pan evaporation show decreases, due to decreases in surface radiation associated with increases in clouds, changes in cloud properties, and/or increases in air…" should be something like "continental-scale pan evaporation is estimated to have decreased due to diminihsed surface insolation associated with increased cloud cover, changes in cloud properties, and/or increased air... [Patrick Minnis]	
3-261	A	4:30	4:33	Ref. comments #2 and #3 above: "However… the actual evaporation becomes closer to the potential evapotranspiration measured by the pans (line 32)." First the terminology: Potential evapotranspiration (ETp) is not measured by pans. For a start, pans don't transpire: they measure only a rate of evaporation, not of evapotranspiration. We call this pan evaporation (Epan). ETp can then be derived from Epan, but these are not the same fluxes. For "actual evaporation," "actual evapotranspiration" is meant. Second, the use of "however" in line 30 implies a contradiction between decreasing Epan and increasing ETa that does not exist. They are complementary fluxes: Epan meters ETp, and ETp is complementary to ETa on a regional scale. In the absence of trends in the radiative dynamic driving ETa, Epan, and ETp, trends in the aerodynamic driver (humidity and windspeed) force ETp and Epan to vary in a complementary manner to ETa: the rates converge as moisture availability increases and diverge as moisture availability decreases. Radiative trends force all ET fluxes and measures (Epan, ETp, and ETa) to respond in the same direction as the radiative trend. When there are trends in both dynamics, the responses in the ET measures and fluxes superpose. As stated in #3, this complementarity is well documented. [Michael Hobbins]	
3-263	A	4:30	4:34	add a reference to a possible increase of solar radiation since 1990 [Bernard Seguin]	
3-264	A	4:31	4:31	Ref. comment #2 above: What is "actual inferred evaporation"? Is it one of many different types of inferred evaporation? What is inferred evaporation, anyway? Can something be actual and still be inferred? Logically, if it's actual, it's no longer inferred. Point is, I have no idea what flux you're talking about here. [Michael Hobbins]	
3-266	A	4:31	4:31	"actual inferred evaporation" appears to be an oxymoron, the entire sentence is weak, need a clearer explanation because a policy maker will see this as gibberish unless "actual inferred" can be explained satisfactorily. [Patrick Minnis]	
3-267	A	4:32	4:32	replace "evapotranspiration" by "evaporation" (evapotranspiration is not measured by pans because it contains also the transpiration from plants) [Reinhard Böhm]	

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3-268	A	4:32	4:34	I don't understand these two sentences. Some clarification would seem to be required here, for an executive summary. And isnt "actual inferred evaporation" a contradiction in terms? Shouldn't it be "inferred actual evaporation"? [Neville Nicholls]	
3-269	A	4:33	4:33	Ref. comment #2 above: "Hence there is a trade-off on evaporation…" replace "evaporation" with "actual evapotranspiration." [Michael Hobbins]	
3-271	A	4:33	4:33	is a trade-off on evaporation between less solar" should be something like "is an evaporation trade-off between reduced solar [Patrick Minnis]	
3-272	A	4:36	4:37	As shown later in this chapter, the changes in cloud cover are complex and variable. [Marcia Baker]	
3-273	A	4:37	4:38	a lot of hypothesis do not stand up to independent data. Why focus on cosmic rays hypothesis in executive summary? [Jean-Marc Moisselin]	
3-274	A	4:37	4:38	This statement about attribution might well be shifted, with its evidence, to Chapter 9. But I am happy for it to be left in this chapter. [Neville Nicholls]	
3-275	A	4:37	:38	This statement is incorrect and misleading. More recent ground measured data suggests there is a significant yet weak association between cosmic rays and cloudiness. See the recent paper by R.G. Harrison and D.B. Stephenson, 2005: Empirical evidence for a non-linear effect of galactic cosmic rays on clouds,Proceedings of the Royal Society of London. Series A, submitted. R.G. Harrison and D.B. Stephenson, 2005:Empirical evidence for a non-linear effect of galactic cosmic rays on clouds,Proceedings of the Royal Society of London. Series A, submitted. [David Stephenson]	
3-276	A	4:38		Besides, there is no trend in the cosmic ray record (eg Richardson et al., 2002, JGR, vol 107., doi: 10.1029/2001JA000507; Benestad 2005, GRL, 32 L15714, doi:10.1029/2005GL023621) [Rasmus E. Benestad]	
3-277	A	4:39	4:40	Ackward sentence: "…possibly El Niño Southern (ENSO) Oscillation related in part…" [Henry Diaz]	
3-278	A	4:39	4:40	possibly El Niño Southern Oscillation (ENSO) related in part" should be "possibly related in part to the El Niño Southern Oscillation (ENSO)	

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				[Patrick Minnis]	
3-279	A	4:40	4:40	How "related"? Which is cause and which effect? If you're saying that El Nino has caused changes in the radiation at the top of the atmosphere, you might want to specify LW. If you don't mean LW, then the cause has to be the radiation, and the effect El Nino, no? [Michael Hobbins]	
3-281	A	4:44	4:44	I would prefer "(which is largely determined by a few major patterns and modes)". [Neville Nicholls]	
3-282	A	4:44	4:45	Can you squeeze in a (< 1 sentence) definition of annular modes? Many readers will not know what these are. [Neville Nicholls]	
3-283	A	4:44	4:45	I would drop "and annular modes". Annular modes are just one type of major pattern. Other types of pattern are things like the PNA and NAO. Or use something like "major wave-like teleconnection patterns and annular modes" [Adrian Simmons]	
3-284	A	4:44	5:22	There are some straightforward model-based theoretical arguments that suggest a robust dynamical basis for expecting a "tightening" of the Arctic and Antarctic "annular modes" as earth contiues to get warmer and wetter. I would be pleased to discuss this with the contributing or lead authors, if they so wish. [Jerry Mahlman]	
3-285	A	4:44		What does “relatively” few mean? Does this mean several? Imprecise words like “relatively” , “reasonable” , “storm activity” (page 4, line 52) should be removed from the Chapter text. [David Stephenson]	
3-286	A	4:45	3:45	The phrase "confidence is greatest after 1979" is confusing because it's unclear what it refers to. [Dian Seidel]	
3-287	A	4:51	4:51	analysis" should be "analyses [Patrick Minnis]	
3-288	A	4:54	4:55	The influence on the PDO on ENSO is overstated here. I don't think there is a concensus about the causes of changes in ENSO over decadal time scale. [LuAnne Thompson]	
3-289	A	5:5	5:7	I find this sentence a bit misleading. The SAM has changed most during the summer season, yet the greatest warming has been on the western side of the Antarctic Peninsula during winter. This sentence implies that the warming is a result of the SAM shifting into its positive phase. [John Turner]	

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3-291	A	5:7	5:7	change appear to may [LuAnne Thompson]	
3-292	A	5:10	5:22	please add the respective reference periods if You state things like "most active storm year", "considerably above normal", "second highest on record". Even in a summary like this such simplifications are not acceptable. [Reinhard Böhm]	
3-293	A	5:10	5:22	this paragraph should include information on the frequency and trend in tropical cyclones over the entire 20th century, including the North Atlantic for which reliable data exist. [Howard Feldman]	
3-294	A	5:10	5:22	I suggest to insert a sentence about the anomalous frequency of hurricanes during 2005 in the Atlantic. [Piero Lionello]	
3-295	A	5:10	5:22	I found this "hurricane trend argument" to be unnecessarily obscure. Clearly, we expect the intensity of hurricanes to increase, simply because the tropical ocean temperatures are increasing globally. That was well recognized in pre TARscientific publications. [Jerry Mahlman]	
3-296	A	5:10	5:22	The paragraph refers to tropical cyclones, hurricanes and typhoons until 2004. An update with 2005 data would be informative. [Philippe Tulkens]	
3-297	A	5:10		Please reference this correlation between ENSO and hurricanes. I thought it was relatively weak. [Steven Siems]	
3-298	A	5:12	5:12	What about the 2005 hurricane season? Maybe it was more active than 1997? [Pavel Groisman]	
3-299	A	5:12	5:12	"By far the most active tropical storm year is 1997,..." Specify "globally" or the specific ocean basins. [Chiu-Ying LAM]	
3-300	A	5:14	5:15	Not clear what 'values' referes to - presumably means number of storms? [John Caesar]	
3-301	A	5:14	5:15	This sentence is clunky. What about "El Nino years are usually accompanied by relatively weak tropical cyclone activity in the North Atlantic and stronger activity in the Pacific."? Actually it depends where in the Pacific of course, but I think this detail can be ignored for the urposes of this summary. [Neville Nicholls]	
3-302	A	5:17	5:18	Whether or not the March 2004 South Atlantic storm Catarina was true tropical cyclone, whether it was unique in the historical record, and whether or not it is related to climate	

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				change are all unsettled issues. For example, the water temperatures over which it formed were actually a bit cooler than normal. And further, the Spring 2005 UCAR Quarterly, (http://www.ucar.edu/communications/quarterly/spring05/index.html) reports on efforts by Lance Bosart to assemble a research team in order to look into the historical records to see whether there have been any other Catarina-type storms in Brazil. Not enough mention is made of the difficulties and/or ongoing investigations into assessing the true historical nature of Catarina. [Jeffrey Kueter]	
3-303	A	5:17	5:22	Executive summary: The pheonemon is referred here as hurricane, but in the region there is not a consensus on the name, it is widely names as extratropical cyclne, since hurricanes do not appear on these region of the South Atlantic. [Jose Marengo]	
3-304	A	5:17		occurred" should be moved after "South Atlantic [Steven Sherwood]	
3-305	A	5:18	5:18	To add after 'Brazil': 'and the first recorded tropical cyclone born close to Iberian Peninsula, in Madeira surroundings, in October 2005 (hurricane Vince).' [Javier Martin-Vide]	
3-306	A	5:18	5:18	Replace '1995-2004' by '1995-2005' [Javier Martin-Vide]	
3-307	A	5:18		Insert "much more" before "satellite data". The chapter in general ignores the existence of satellite data before 1979. ERA-40 assimilated VTPR data from 1973. I'm also not sure that the chapter ever properly acknowledges that it was not only the satellite component of the observing system that improved from 1979 onwards. Drifting ocean buoys in the southern hemisphere and increased aircraft data deserve a mention somewhere. Some of it is there buried away in 3.A.5.2, but some of the contributed text was edited out, in particular: "1973 was a key year that saw a significant increase in the number of aircraft observations, the first few buoy observations, and radiances from the first of the VTPR sounding instruments flown on the early NOAA series of operational polar-orbiting satellites. Pseudo surface-pressure observations derived from satellite imagery over the southern oceans provided a further data source." Uppala et al (2005) show that the southern hemisphere ERA-40 analyses improved in 1973 as well as 1979, although the latter change was indeed more substantial than the former. See also Trenberth and Smith (2005). [Adrian Simmons]	
3-308	A	5:20	5:22	Sentence beginning 'The 21 typhoons…' is poorly structure. Replace with something like "2004 had the second highest number of typhoons on record (21) compared to 1997 (23) in the western North Pacific."	

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				[John Caesar]	
3-309	A	5:20	:22	It might also be useful to include 2005 in the discussion of exceptional Atlantic cyclones. [David Stephenson]	
3-310	A	5:21	5:22	Should include the Webster et al finding of a consistent recent trend towards increased frequency of most intense TCs. [Neville Nicholls]	
3-311	A	5:21	5:22	It is noted that the last sentence is not qualified by the likelihood of the substantial upward trend in the potential destructiveness of hurricanes. It would be welcome if some indication of the significance would be included (e.g. likely, very likely). [Klaus Radunsky]	
3-312	A	5:21	5:25	More references needed here. References for the reanalyses (Kalnay et al., 1996; Uppala et al., 2005 should certainly be given. Also, three references by reanalysis-users rather than reanalysis-producers are given concerning discontinuities in the reanalysis products. I think it should be recognised that the reanalysis producers themselves were immediately aware and themselves documented discontinuities. Reference to Kistler et al. (2001) would be appropriate for the NRA and Simmons et al. (2004) would be appropriate for ERA-40. Also, put an e.g. before the list of references as there are other possible references that could be used. Here's somewhere that a Trenberth et al. reference could be dropped if comment 2 above is accepted. [Adrian Simmons]	
3-313	A	5:21		should there be a caution about data reliability and homogeneity or are you confident in this finding? [Gabriele Hegerl]	
3-315	A	5:22	5:23	The 2005 hurricane season in the Atlantic has records for number of storms, and major hurricanes. This item should also be included. [Henry Diaz]	
3-316	A	5:23	5:23	There is no discussion in the Executive Summary on analyses of the poleward transport of energy by the ocean and atmospheric circulations, even though these were referred to in the Overview Chapter 1. Some reference is made in the discussion of El Nino, including enhanced transport on interannual timescales with ENSO and possibly on longer timescales. Trenberth and colleagues have done basic analyses on these variations since IPCC TAR that should be discussed in more detail because of the importance to temperature variations on these timescales. [WILLIAM KININMONTH]	
3-317	A	5:33		It is slightly strange to write "there is an infinite variety of weather systems". There are really only a limited number of types of weather system. Of course, there are variations of intensity, scale, etc, between different realizations of a particular type of system. Some	

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				rewording is necessary. [Adrian Simmons]	
3-318	A	6:1	7:24	The Introduction is very long, wordy and (in my opinion) not necessary. I think it could be replaced by just 3-4 sentences. [Neville Nicholls]	
3-319	A	6:11	6:12	English is not clear. [Marcia Baker]	
3-320	A	6:11		Insert after “trends”; “ for the surface record compiled by the Hadley Climate Center (UK)” [Vincent Gray]	
3-321	A	6:11		Suggest inserting "surface" before "temperature trends" [Dian Seidel]	
3-322	A	6:12	6:12	It seems particularly risky to be ending the first interval in 1945, meaning that the last 6-8 years of the record are from the time during WW II when it is well known that there were significant biases in the temperature records (at least for marine nighttime air temperature, there was a very large positive bias in the observations for which correction has been made in a way that is more hopeful than objective) and significant variations in the distribution of observations around the world (as referred to very briefly on page 8, line 6 and shown in the figures showing ocean anomalies by ocean and latitude). Looking at Figure 3.2.4, both the NH and SH records show quite sharp positive anomalies almost exclusively during the war years (and in particular two years with very positive anomalies) that certainly seem to leave open the possibility that a significant bias remains in the data for this period--and so making this the end of the trend line for the first period may well be creating a misimpression about the intensity of the warming during this period. [Michael MacCracken]	
3-323	A	6:13	:27	It is not obvious to me how the "climate shift" or "new regime" can be pinpointed so precisely to 1976. The fact that temperatures began to rise in 1976 does not mean that physically the anthropogenically influenced regime had not begun earlier. [Fons Baede]	
3-324	A	6:13	:14	The “climate shift of 1976” does not reflect a consensus view of climate scientists. It is neither expected from dynamical arguments (why should there be a step change to small incremental increases in greenhouse gases?) and it would certainly not stand up to a rigorous change point statistical analysis. It reflects one of the lead author’s pet ideas already discussed in the previous IPCC 2001 report. It is inappropriate to use this assessment document, to advertise the authors’ own speculative ideas.	

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				[David Stephenson]	
3-325	A	6:14	6:16	Delete from "and seems to" on line 14 to IPCC.2001) on line16. This is a truly outrageous statement. How could you expect anybody to believe that the greenhouse effect could decide to start at a particular date? Is is divine intervention? [Vincent Gray]	
3-326	A	6:15	6:15	As written it suggests an entire attribution. Insert "partially" after the word "been" and before "attributed". [Ross McKitrick]	
3-327	A	6:16	6:17	Delete from "The picture" in line 16 to "here" in line 17. The statement is unnecessary [Vincent Gray]	
3-328	A	6:17		Replace "However, it proves to be " with "It is" [Vincent Gray]	
3-329	A	6:18	6:27	It may be worth mentioning here that the first conclusion about "global warming" was published in refereed literature exactly in 1976 (Budyko and Vinnikov, 1976, "Global Warming", in Russian Meteorol. and Hydrol.), although their conclusion was based on the surface air temperature data up to 1975 for the Northern Hemisphere only. [Pavel Groisman]	
3-331	A	6:21	6:27	This text is at very significant odds with many other places in the text where the obvious limitations of reanalyses for the particular exercise of long-term change analysis are expounded again and again and again for a whole suite of variables. The text needs to be modified to clarify that while useful for process and short-timescale analyses, the current generation of reanalyses contain discontinuities which compromise their utility for characterising long-term changes. This applies throughout reanalyses and is not limited to the single effect of the satellite stream coming on-line as implied at present, although this is undoubtedly the most obvious problem. Also, ERA-40 ingest satellite data prior to 1979. The similarity in surface trends (used elsewhere in the chapter [page 10 lines 28 to 31] to support this argument) is an insufficient justification for the subsequent application to other variables because surface temperature is a weak (insufficient) constraint on the climate system evolution. And the similarity in trends for MSU2 is largely fortuitous as detailed elsewhere in my review. [Peter Thorne]	
3-332	A	6:23	6:27	I find this subsection a bit obscure. There are appeals to the 1976-79 "climate shift" without describing phenomologically the cause, or the manifestations, of this "shift". Can this be clarified as to its climate warming assessment significance? What occurred that caused the shift? How global was it and why is it so seldom discussed, or, seemingly, diagnosed? Was it ENSO related?	

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				[Jerry Mahlman]	
3-333	A	6:24	6:26	English requires improvement [M James Salinger]	
3-334	A	6:25	6:25	suggest adding reference to Bengtsson et al.(2004) which is included in ref. list [Richard Allan]	
3-335	A	6:25	6:25	Insert word 'high' before 'quality'. [Marcia Baker]	
3-336	A	6:29	7:6	These two paragraphs are almost meaningless. Delete them [Vincent Gray]	
3-337	A	6:30	6:30	"Climate varies on all… time-scales" A variation in the climate at very short time-scales is merely weather. [Michael Hobbins]	
3-339	A	6:31	6:32	Replace “anomalies (departures from normal) with “departures from the average”. You do not know what is “normal” An “anomaly” means something that departs from the usual [Vincent Gray]	
3-340	A	6:33	6:33	“Although there are an infinite variety of weather system…” This statement is scientifically unsound. We know climate system is controlled by certain governing equations which we learned from dynamics (text book by Holton). Then the system can have huge, but limited, ways in variety. [Menglin Jin]	
3-341	A	6:33		Replace “are” with “is” Grammar! [Vincent Gray]	
3-342	A	6:35		Replace “anomalies” with “variabilities” [Vincent Gray]	
3-343	A	6:46		Delete “enormous” .Don't exaggerate [Vincent Gray]	
3-344	A	6:47	6:47	A high number of record-breaking events is a sign of changes in the pdf (quite profound implications!). [Rasmus E. Benestad]	
3-345	A	6:47		Replace “extremes“ with ”they are”, cancel full stop, lower case for “But” [Vincent Gray]	
3-346	A	6:48	6:50	Confusing sentence [Galina Churkina]	
3-347	A	6:50	6:50	But the response locally” should be “But the local or regional response… [Menglin Jin]	

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3-348	A	6:51	6:51	line within parentheses seems clumsy. An alternative, possibly equally clumsy, suggestion is: "(for example, global warming modifies planetary waves in the atmosphere that induce a local cooling)" [Richard Allan]	
3-349	A	6:53	6:55	In this sentence and in Table 3.1, it's unclear what the statistical measures of temperature variability are derived from. Is it hourly data for Boulder and monthly data, spatially averaged, for the globe and the US? [Dian Seidel]	
3-350	A	6:54	6:55	4-times the standard deviation" could be misunderstood, please correct to "+ - 2 times the standard deviation [Reinhard Böhm]	
3-351	A	6:54	6:55	To be consistent, please use the term +/- two times the standard deviation (compare e.g. p. 7, line 15) instead of "four times ..." and note that not all data are Gaussian distributed (only in this special case 95 % of data are found within these thresholds of standard deviation). [Christian-D. Schoenwiese]	
3-352	A	6:54	:56	The limits +/- 2 standard deviations contain 95.5% of the observations NOT 95% of the observations if normally distributed. I think the authors should use 1.96 rather than 2 for all their intervals throughout the chapter and then they would be able to quote 95% confidence intervals rather than the non-standard 95.5% ones. I don't understand what is meant by the statement "A normal distribution is a reasonable approximation in most places for temperature …". For most locations, it can be easily shown that temperatures are invariably NOT normally distributed due to skewness in either the warm or cold tails. So what is meant by "reasonable"? Reasonable for what?? The authors should explain what exactly is meant by the word "reasonable" whenever they use it throughout the chapter. The normal distribution is certainly not reasonable if one wants to work out return periods of extreme temperatures based on the historical data! [David Stephenson]	
3-353	A	6:56	7:1	it is not only the continental interiors but the greatest parts of the continents and the skewness is also given for summers (positvely skewed to the warm tail of the frequency distribution) [Reinhard Böhm]	
3-354	A	7:0		Clarify in Table 3.1. that the daily (monthly) anomalies have the diurnal (annual) cycles removed. [Qiang Fu]	
3-355	A	7:0		Section 3.2 Changes in Surface Climate: Temperature This section lacks a thorough discussion on the impacts of land/use change. Dr. Roger	

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				Pielke, Sr. is a leading scientist in the field of the impacts of land use change on the local, regional, and global climate, yet, none of his research on the topic is included in this section. Dr. Pielke suggests the following in regards to how land use changes and their impacts be included in the IPCC AR4: As recognized by the National Research Council in 2005 (Radiative Forcing of Climate Change: Expanding the Concept and Addressing Uncertainties) land-use/land-cover change is a first-order climate forcing. However, its role as a regional and global climate influence is not widely recognized, except as it effects the atmospheric concentration of carbon dioxide and the global average surface albedo. In the summary figure from the IPCC TAR Statement for Policymakers, in terms of the global mean radiative forcing, only albedo effects of land use/land cover change are identified. However, numerous studies have shown that the effect of land-cover/land-use change is to alter temperatures and precipitation in regions where the change occurs, as well as weather globally through teleconnections (see, for example, Pielke, R.A., et al., 2002, Phil. Trans. R. Soc. Lond. A, 360, 1705-1719, and Marland G., et al., 2003, Clim. Pol., 3, 149-157). We should, therefore expect global climate effects from land-use/land-cover change. The next IPCC needs to focus more on this first-order climate forcing than they have in the past. The question of searching for a "discernable effect on the climate system" misses the obvious in that we have been altering regional and global climate by land-use/land-cover change for decades. The issue of land use change is so important that it should be afforded its own subsection under Section 3.2, rather than as an afterthought in subsection 3.2.2.2 (Urban temperature and urban heat island). [Jeffrey Kueter]	
3-356	A	7:0		Section 3.2.1: a relevant and detailed work has been performed about the changes in the surface temperature, but in the introduction it could be helpfull to shortly describe the theoretical frameworks underlying the observed temperature changes (see for instance figure 2.2.2). [Paolo Michele Ruti]	
3-357	A	7:0		Table 3.1 Perhaps this table should include one more entry for "Global mean annual anomalies", especially since a number is given later in the report for the maximum difference between consecutive years, as a benchmark for comparison with long-term trends. If the different datasets differ as to the degree of variability, a range could be given.	

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				[Steven Sherwood]	
3-358	A	7:1	7:2	5. I cannot understand this sentence on line 1, page 3-7 “For the global mean the approximation is somewhat affected by the observed trend, which increases this estimate of the range slightly.” [Menglin Jin]	
3-359	A	7:8	7:13	I simply cannot see the point of this Table. It concerns an utterly unrepresentative single station for an unrepresenttaive time. What's more, there is no literature reference. Delete it. [Vincent Gray]	
3-360	A	7:8	7:13	Table 3.1 should be appended with two lines presenting the Northern and Southern Hemisphere mean monthly anomalies (to avoid the perception that global mean temperature range is negligible only because the seasonal cycle was suppressed (in fact, it is modulated by the Northern Hemisphere). I also would be more comfortable, if together with the United States monthly anomalies, the table contains a similar array of numbers let's say for Australia to avoid the hemispheric bias. [Pavel Groisman]	
3-361	A	7:8	7:13	Boulder is a great place, especially for atmospheric scientists, but at 1700 m altitude and just east of the Rockies, is it even "somewhat" reperesentative ? If complete datasets are readily available, I suggest that Des Moines (Iowa) and Kiev (Ukraine) or Potsdam might be more representative of mid-latitude mid-continental sites where people live and food is produced. [Robert KANDEL]	
3-362	A	7:8		Replace “somewhat" with “completely' [Vincent Gray]	
3-363	A	7:8	:13	Table 3.1. This table could be made a lot clearer by having 3 columns for space (Boulder, US average, global average) and 5 rows for time scale (hourly mean, daily mean, monthly, annual, decadal) and then use the same statistic (e.g. standard deviation) for each of the 15 cells. As it is, the table mixes up time and space scales in a single column. [David Stephenson]	
3-364	A	7:9	7:9	mid-continental mountainous station... (plus put the elevation of Boulder, Colorado in the caption). [Pavel Groisman]	
3-365	A	7:9		Replace “representative” by “unrepresentative” [Vincent Gray]	
3-366	A	7:12	7:13	The precise meaning of the table entries is not clear. Anomalies are not the same as	

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				excursions. No uncertainties are given. [Marcia Baker]	
3-367	A	7:14	7:24	It would be good to put this paragraph nearer the beginning of the chapter, and highlight it. [Marcia Baker]	
3-368	A	7:14	7:24	This paragraph id entirly commendable, and should apply to every Chapter. It should appear in a General Introduction. It needs to be applied particularly strongly to Chapter 5 [Vincent Gray]	
3-369	A	7:14	7:24	I am complimented by IPCC's slightly modified use of my simple "betting odds" scheme(Mahlman, J.D., SCIENCE, 1997, Vol. 278, pp. 1416-1417). It is, however, a good example of how key advances in how assessment conclusions can be adopted without any subsequent responsibility to the original usable and referencable contribution. An amusing different example in this Chapter is a post-TAR literature reference to an author who defined "Rossby Waves", as if for the first time! [Jerry Mahlman]	
3-370	A	7:16	7:17	"... given the null hypothesis" not clear. I suppose that it is intended to say that in case of the null hypothesis we see no trend or any other peculiarity but otherwise we see at any defined confidence level. [Christian-D. Schoenwiese]	
3-371	A	7:17	7:17	It would be worth to introduce also a statistically sound definition of trend [Franco Desiato]	
3-372	A	7:18	7:18	to" should be inserted after "… equivalent [Galina Churkina]	
3-373	A	7:20	7:24	I think Martin Manning told us that these IPCC definitions have been updated, so that very likely means > 90%, rather than >90% but less than 99%. Similarly likely is > 66%, unlikely < 33% etc. [Nathan Gillett]	
3-374	A	7:20	:24	In defining judgemental estimates of confidence, the authors are reinventing the wheel here. Furthermore, their arbitrary definitions do not match those used by scientists in publications. The authors should consult the interesting survey published by Frederick Mosteller; Cleo Youtz, Quantifying Probabilistic Expressions, Statistical Science, Vol. 5, No. 1. (Feb., 1990), pp. 2-12. This survey found that very likely meant a probability of 0.85, likely meant 0.69, and unlikely meant 0.16 on average. [David Stephenson]	
3-375	A	7:21	7:24	Suggest providing the information in a table rather than as text.	

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				[Lourdes Maurice]	
3-376	A	7:30	8:16	It seems quite unfortunate that there is apparently not work going on (or at least described here) to go back and carefully recheck the observations during the years of WW II, when there were significant positive anomalies that seem quite unusual (after all, the diminution of volcanic aerosols occurred by about 1915 and the solar increase was more gradual than the sharp upward shift in temperature shown in Figure 3.2.4--especially evident in the SH, which is dominated by oceans and the temperatures of which are relatively hard to change. It would really seem that there should be further looks taken at the adjustments to observations that have been made to see if they are sufficient. The notion that so much of our understanding of the 20th century rests on the significantly degraded set of observations from during WW II--and that this is not even being mentioned--seems to me to be a serious omission in the report and by the research community. That the record since WW II, when we have much better data shows nothing like the kind of several year jump as occurred during WW II, and that the fluctuation was greater over the ocean than over the land areas should both be raising our suspicions about this interval of the record--and to then use this period as a breakpoint in the analysis of the 20th century temperatures seems quite risky. [Michael MacCracken]	
3-377	A	7:38	7:38	Should clarify as "A new gridded dataset of monthly maximum and minimum temperatures…." [John Caesar]	
3-378	A	7:43	7:45	Might also mention the new work by Chelton and Wentz (2005) using microwave SSTs "Global Microwave Satellite Observations of Sea Surface Temperature for Numerical Weather Prediction and Climate Research" Bulletin of the American Meteorological Society: 86, 1097–1115. [Chris Folland]	
3-379	A	7:47	7:47	What is skin temperature? Who's skin is it? [Galina Churkina]	
3-380	A	7:47	7:47	I am not sure that all readers will interprete correctly the term "skin (temperature)"; brief explanation recommended. [Christian-D. Schoenwiese]	
3-381	A	7:48		the sentence “These data represent skin temperature, not air temperature, and so must be adjusted to match the latter” is misleading. The differences between skin and air temperatures are determined by various processes including land cover, boundary condition, soil moisture and clouds condition. There is no way to “adjust” skin temperature to air temperature. More importantly, the skin temperature work is an additional evidence to surface warming, which were traditionally detected only from	

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				surface air temperature. Strongly suggest a. to revise this sentence to: These data present skin temperature, an independent variable closely related to surface radiation properties. It prove sunique, additional evidence for surface warming (Jin 2004). b. add reference Jin 2004 in this sentence: Jin, M., 2004: Analyzing Skin Temperature variations from long-term AVHRR Bulletin of the American Meteorological Society, vol 85, No. 4, 587-600. This paper is very critical in reviewing skin temperature character, shortcoming, uncertainty, and global mean warming. It has been reported by over 60+ media. [Menglin Jin]	
3-382	A	8:12	8:12	Define 'climate quality' data (also, p. 117, line 7). [Marcia Baker]	
3-383	A	8:13	8:13	NRA is not defined on first usage. [Henry Diaz]	
3-384	A	8:13	8:13	Define "NRA". [Qiang Fu]	
3-385	A	8:13	8:14	6. Reference is needed for the sentence on line 14, page3-8: " Improvements in the ERA-40 over NRA arose both from improved data sources and better assimilation techniques, although problems remain (what problem remains, need new REFERENCE)" [Menglin Jin]	
3-386	A	8:17		Insert " McKitrick and Michaels (2004) have shown that the gridded surface data still contian significant biases from such socioeconomic factors as chnages in income, GDP, and coal usage, all indicators of urbanization. They also found serious deficiencies in data from the former Soviet Union between 1990 and 2001 [Vincent Gray]	
3-387	A	8:20	8:45	I noticed that glob. mean. T for 1956 appears to be a deal lower now (~ -0.6C) than in TAR (~ -0.4C). Presumably this can be accounted for by new data? [Rasmus E. Benestad]	
3-388	A	8:20	8:45	Please, include results of the Russian global temperature time series (Vinnikov et al. 1990, J.Climate) that is routinely updated to 2004 and is publicly available from Lugina et al. (2005, cited above). This is especially relevant because the paragraph discusses optimal averaging etc. that was used in the Russian data set and ignored in other time series. The update of Figure 3.2.1 and Table 3.2 (and/or 3.3) will also be needed to	

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				accomodate the above suggestion. [Pavel Groisman]	
3-389	A	8:20	8:57	I think that there are real concerns about reanalysis reliability of "weather-data time series". If there are strong new accomplishments with this spotty and glitch-filled data sets, I think that the text needs to be much clearer as to how these reanalysis data sets are now of climate trend quality. [Jerry Mahlman]	
3-390	A	8:21	8:22	When Brohan et al (2005) is used, CRUTEM3(v) should replace CRUTEM2v. CRUTEM3(v) starts 11 years earlier, in 1850. [Chris Folland]	
3-391	A	8:21	:45	Please add table explaining the many, many acronyms. Very difficult to read! [Fons Baede]	
3-392	A	8:22	8:24	I have been bothered by this since serving on the FAR and SAR. Jones et al. construct the "global" land Tsfc from equal weights of the NH and SH. I believe it should be areally weighted (i.e. so that the NH has more weight in determining the global land surface temp because it has a lot more land.) [John R Christy]	
3-393	A	8:22		What is the v in CRUTEM2v - is this in some form variance corrected? Or otherwise corrected? Should that be mentioned? [Gabriele Hegerl]	
3-394	A	8:23		surface of land in NH is more greater than in SH, the temperature trend of the globe is probably not the mean of temperature trend of NH and SH but weighting by the surface land of each Hemisphere [Matari Amar]	
3-395	A	8:23		if we consider the surface of land in NH is more greater than land SH, the temperature trend of the globe is not the mean of temperature trend of land NH et land SH but weighting by the surface of land of each Hemisphere.. [Amar Matari]	
3-396	A	8:26	8:27	Change "The NCDC analysis." with "The trend estimated from NCDC analysis ". [Aristita Busuioc]	
3-397	A	8:28	8:28	Arguably non infilled data should be used for the reasons given as Fig 3.2.1 exaggerates the real disagreements. [Chris Folland]	
3-398	A	8:29		Replacing "Most infilling" with "Such interpolation" might improve the connection between this and the previous sentence, leading to increased clarity. [Neville Nicholls]	

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3-400	A	8:31	8:45	Although unable to offer specific suggestions I found this section particularly difficult to follow. There are too many acronyms and the assumption is that the reader will understand these. Being au fait with the subject and acronyms I struggled so I am somewhat uncertain what a non-expert would gain from this text. While undoubtedly important I think it can be coined in a more audience-friendly manner. [Peter Thorne]	
3-401	A	8:39	8:39	Change "Historic" to "Historical" [Henry Diaz]	
3-402	A	8:41		correct: "use" to "uses" [Hartmut Grassl]	
3-403	A	8:42	8:42	Should read 'long-term', not 'long-tern' [John Caesar]	
3-404	A	8:42	8:42	Change "-tern" to "-term". [Qiang Fu]	
3-405	A	8:42	8:42	long-tern trends" instead of "long-term trends [Jean-Marc Moisselin]	
3-406	A	8:42	8:42	It should be "term" instead of "tern" [Eugene Rozanov]	
3-407	A	8:45		Actually, the technique "gives EQUAL weight to the less well-covered SH". [Neville Nicholls]	
3-408	A	8:47	8:52	This seems to be another disconnect in the text between the written word and the figures (3.4.3). The large-area trend values of ERA-40 do not agree with any of the surface trends as shown in the figure. CCSP comments on this. [John R Christy]	
3-409	A	8:47	8:52	High interannual correlations don't prove trends are similar. State one or two ERA-40 trends. [Chris Folland]	
3-410	A	8:47	8:52	Here you are comparing trends from the ERA40 and surface station data, showing a good agreement. However, are the two datasets independent? I thought the station data are assimilated into the ERA40 reanalysis system. [FILIPPO GIORGI]	
3-411	A	8:47	:52	Fig 3.2.1. Why is the ERA-40 analysis not shown in fig 3.2.1 [Fons Baede]	
3-413	A	8:49		what are the correlations - of smoothed data? How well do the trends agree? [Gabriele Hegerl]	
3-414	A	8:55	8:56	The daily minimum and maximum temperature trends are 0.20 K/dec and 0.14 K/dec.	

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				This would lead one to think that the DTR trend should be -0.6 K/dec, not -0.7 K/dec (as reported). Perhaps this is an issue of where numbers are boing rounded off or how the trends are calculated, but these numbers look a bit strange and need clarification. [FILIPPO GIORGI]	
3-415	A	8:57	9:3	I found this coverage discussion really confusing, and I'm not sure of the utility of it. It may be better to map in some way if it is retained. This would also give a more intuitive feel for the non-stationarity of the observaing system. At present this is not clearly articulated. A series of coverage maps by decades almost as postage stamps may help? [Peter Thorne]	
3-416	A	8:57		"71% of the terrestrial surface (compared with 54% in the TAR)" might be clearer. [Neville Nicholls]	
3-417	A	9:0	13:	NH SST,UKMO 0,155 SH SST,UKMO 0,140 The value of the globe is 0,153, This value must be near 0,145 [Matari Amar]	
3-418	A	9:0		possible contradictions in temperature trend see table 3.3 and 3.3 [Matari Amar]	
3-419	A	9:0		For 1901-2004: SH Land CRU 0,073 SH SST,UKMO 0,068 and SH (CRU,UKMO) of table 3,3 is 0,061 smaller than the two hemisphere [Matari Amar]	
3-420	A	9:0		NH(CRU,UKMO) 0,072 SH(CRU,UKMO) 0,061 and globe(CRU,UKMO)0,062 [Matari Amar]	
3-421	A	9:0		For 1910-1945: NH land CRU0,135 NH SST,UKMO 0,155 and NH(CRU,UKMO) of table 3,3 is 0,165 greater than NH land and NH SST, the value must be near 0,145 [Matari Amar]	
3-422	A	9:0		Discussion of land use change effect on temperature is very poor. Read Chapter 7 (esp. 7.1.1)! [Galina Churkina]	
3-423	A	9:0		The temperature trends over lands in Table 3.2 from GISS are quite different from those from CRU and GHCN after 1979. Some explanations may be needed here. [Qiang Fu]	
3-424	A	9:0		Many contradictions are in temperature trends, see table 3.2 et 3.3. Exemples : For 1901-2004 SH Land, CRU 0.073 SH SST, UKMO 0.068 and SH (CRU, UKMO) of table 3.3 is 0.061 smaller than the two hemisphere. NH (CRU,UKMO) 0.072 SH (CRU, UKMO) 0.061 and globe (CRU,UKMO) is 0.062.	

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				For 1910-1945 NH Land CRU 0.135 NH SST,UKMO 0.155 and NH (CRU,UKMO) of table 3.3 is 0.165 greater than NH Land et NH SST. The value of NH(CRU,UKMO) must be near 0.145. NH SST,UKMO 0.155 SH SST,UKMO 0.140 the value of the Globe is 0.153 This value must be about 0.145 [Amar Matari]	
3-425	A	9:0		Table 3.2. Here, and in Appendix 3.A.1.2, reference is made to "Restricted Maximum Likelihood" standard errors, but the citation is to a general text book (Diggle), not a published article. Considering the importance of the contents of this table to the Chapter, the reader needs considerably more guidance about the estimating methodology, as well as reference to current literature. There is a substantial literature dating back to the early 1990s showing that anomaly data have long autocorrelation processes in them, making for long term persistence and near unit-root behaviour. It is well known in the climate literature that this can severely bias significance estimates in trend regressions. Yet there is no mention of this problem and it seems that the t-stats in this table reflect only a first-order autocorrelation correction, almost certainly making them misleading. I will suggest some improved wording, but I believe this table needs a serious re-do and the reader is owed a substantial discussion of the problems of estimating significance of trends in climatic data. Below I cite a forthcoming treatment of the issue by Cohn and Lins, who comment "It is therefore surprising that nearly every assessment of trend significance in geophysical variables published during the past few decades has failed to account properly for longterm persistence.... For example, with respect to temperature data there is overwhelming evidence that the planet has warmed during the past century. But could this warming be due to natural dynamics? Given what we know about the complexity, long-term persistence, and non-linearity of the climate system, it seems the answer might be yes." All the trends should be re-estimated using, at minimum, an ARMA(1,1) model, not an AR(1) model; and the lag processes need to be extended out to sufficient length to ensure the ARMA coefficients become insignificant. The treatment of this key issue in this chapter is at least 10 years behind the state of the art (see, for instance, Woodward and Gray JClim 1993, who were already ahead of where this discussion is), and unless substantial improvement is made this Table and related discussions should be removed altogether. [Ross McKitrick]	
3-426	A	9:0	:7	Table 3.2 Additional to the above, why is the estimation done on annual averages, rather than on monthly averages? Annual averaging is very unusual in this context and removes considerable information about the autocorrelation function from the data set; nor is it necessary.	

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				[Ross McKitrick]	
3-427	A	9:0		table 3.2 : 0.076+/-0.055 (SH NMAT, UKMO for 1946-1978) seems to be significant <5% : should be in italic? [Jean-Marc Moisselin]	
3-428	A	9:0		Table 3.2: Proposal: Add column 1901-2000 (see comment 7); same Table 3.3, p. 13. [Christian-D. Schoenwiese]	
3-429	A	9:1		What does "somewhat" under-represented mean here? Is it under-represented or isn't it? The chapter should be carefully proof-read to remove such ambiguous dead wood. [David Stephenson]	
3-430	A	9:2	9:3	Does this inadequate coverage just refer to prior to 1950? I don't think so, but the juxtaposition of this sentence and the previous one suggests this is what you are saying. [Neville Nicholls]	
3-431	A	9:7	9:7	In light of the above, after line 7 insert the following: "Table 3.2 provides trend estimates from numerous climate databases. It should be noted that determining the statistical significance of a trend line in geophysical data is difficult, and most forms of bias in such time series will tend to overstate the significance. Zheng and Basher (1999), Cohn and Lins (2005) and others have used time series methods to show that failure to properly treat the pervasive forms of long term persistence and autocorrelation in trend residuals can make erroneous detection of trends a typical outcome in climatic data analysis." References for the above: Zheng, Xiaogu and Reid E. Basher (1999). "Structural Time Series Models and Trend Detection in Global and Regional Temperature Series." Journal of Climate 12, 2347-2358. Cohn, Timothy and Harry J. Lins (2005). "Nature's Style: Naturally Trendy." Geophysical Research Letters, Accepted and Forthcoming, Fall 2005. [Ross McKitrick]	
3-432	A	9:10	9:16	Proposal: Add 1901-2000 trend values which refer to the secular change and can be easily transformed into trend values per decade. Similar Table 3.3, p. 13. [Christian-D. Schoenwiese]	
3-433	A	9:10	:14	Throughout this chapter, the authors have attempted to assess statistical significance of linear trend estimates by quoting standard errors based on REML estimated linear trends. They describe this approach in Appendix 3.A.1.2 but fail to mention how exactly the approach takes account of serial correlation in the residuals. The Diggle (1999) reference is to a whole book on longitudinal data analysis and REML is only a method for estimation – the trend model needs to be clearly defined here. For example, did the authors assume an AR(1) model for the residuals and no serial dependence between observational errors? The description of the method is incomplete and so does not allow other scientists to reproduce their trend estimates. If an AR(1) model was assumed then this is inadequate for describing longer term decadal variability that occurs in aggregate	

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				series such as hemispheric mean temperature records and NAO etc. – the residuals have more serial dependency than can be captured by a naïve AR(1) model and so the standard errors will be underestimated. [David Stephenson]	
3-434	A	9:12		add a comma after "1999)" [Hartmut Grassl]	
3-435	A	9:14	9:14	I'm not sure what an intrinsic uncertainty actually is. Every effort should be made to avoid complexity in terminology. This relates back to my point regarding the confusion over the uncertainty terminology employed. We should stick, where possible, to widely understood terms. From a holisitic science community point of view rather than climate niche point of view these are: structural, parametric, and statistical. See Thorne et al. (2005, Oct, BAMS) or the CCSP stats appendix. [Peter Thorne]	
3-436	A	9:15	9:18	Table 3.2. The UKMO SST data should now be from HadSST2, Rayner et al (2005). [Chris Folland]	
3-437	A	9:15	9:18	Table 3.2. Say in line 14 intrinsic annual uncertainties were neglected because not available. It would be good to correct this situation for the next draft where possible. [Chris Folland]	
3-438	A	9:15	9:16	The differences of trends among the three data sets as shown in table 3.2 could not be played down, and the reasons for the differences should be assessed. [Guoyu REN]	
3-439	A	9:15		table 3.2: Are the trends in the data consistent between datasets? It's a bit hard to determine from the table, since the uncertainty is for the trend given internal variability, not given data uncertainty, right? Is there any way to determine if they are consistent given that both should show the same realizations of internal variabiity? [Gabriele Hegerl]	
3-440	A	9:15		same table: For chapter 9, it would be great if the second half of the 20th century trends were given rather than starting in 1946. [Gabriele Hegerl]	
3-441	A	9:15	:16	Table 3.2 and all other places where linear trend estimates are quoted. Firstly, the word "linear" should be clearly stated before "trend" when describing such estimates (e.g. in the table caption). Secondly, although the authors attempt to assess the statistical significance of the trends by using what they call "significance" (for what should be referred to as p-values), they fail to say how much variance is explained by the trends (the R-squared coefficient of determination). As well as knowing whether a trend has a statistically significant slope different from zero, it is also essential to know how much of the total variance is explained by the trend. One can have trends that are statistically	

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				significant yet explain very little of the total variance, or alternatively have trends that are not statistically significant yet explain a lot of the total variance. In other words, the authors should give some estimate of the magnitude of the trends compared to that of natural year-to-year variability. It is recommended statistical practice to quote both R-squared and p-values when discussing trend fits. This would help provide the necessary context for proper interpretation of the estimates. [David Stephenson]	
3-442	A	9:19	10:47	A key question in detection of climate change is to what extent the increased urban heat island effect has affected the observed rising of global or regional average surface air temperature. We investigated into the effect of urbanization on area-averaged surface air temperature trend as obtained from RB stations in north China, and the result shows that the effect of urbanization on observed increase in anuual mean surface air temperature of national reference and basic stations in North China is very obvious, reaching 0.11C/10a for 1960-2000 period, accounting for 38% of the total warming recorded.(please see Zhou, Y. Q. and Ren, G. Y., 2005, Identifying and Correcting Urban Bias for Regional Surface Air Temperature Series of North China over Period of 1961-2000, Climate and Environmental Research, 10 (3) (in press in Chinese)) [Guoyu REN]	
3-443	A	9:19	10:47	Other works also show a more significant urban or land use-induced warming in eastern China (please see Zhou L, Dickinson RE, Tian Y, Fang J, Li Q, Kaufmann RK, Tucker CJ and. Myneni RB. 2004. Evidence for a significant urbanization effect on climate in China. Proceedings National Academy of Sciences 101, 9540-9544; Portman, D. A., Identifying and correcting urban bias in regional time series: surface temperature in China's northern plains, J. of Climate, 1993, 6, 2298-2308; Chu, Z. Y., Ren, G. Y., 2005, Effect of enhanced urban heat island magnitude on average surface air temperature series in Beijing region, Acta Meteorologica Sinica, 63 (4), 534-540) [Guoyu REN]	
3-444	A	9:19		Section # 3.2.2.2 The title of the section refers to the UHI effect, although the section deals with both the effects of urbanization and land use changes on temperature.The title has to be modified to better reflect the content. [Galina Churkina]	
3-445	A	9:19		Section 3.2.2.2 Although there is more discussion here than in TAR about the little effect of urbanization on the global warming trend, in my opinion some doubts remain. [Javier Martin-Vide]	
3-446	A	9:19		This section on urban temperatures should acknowledge the spatial patchiness of urban influences and recognize that there heat island is not a monolithic structure but varies depending on land use and local heat production within urban areas (e.g., business	

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				districts vs industrial zones vs parks vs residential). [Dian Seidel]	
3-447	A	9:20	9:20	relative warmth' should be replaced by 'temperature increase'. [Marcia Baker]	
3-448	A	9:20	9:20	should be “Urban Heat Island Effect (UHI, Jin et al. 2005)”, not “Urban Heat Island”. Jin, M, R. E. Dickinson, and D-L. Zhang, 2005: The footprint of urban areas on global climate as characterized by MODIS. Journal of Climate, vol. 18, No. 10, pages 1551-1565. [Menglin Jin]	
3-449	A	10:0		Where does the text describe Fig. 3.2.3? Is this figure inserted in the correct place on page 3-10? [Melinda Marquis]	
3-450	A	10:5	10:6	At least a few references for "many local studies demonstrating that microclimate within cities is warmer …" should be added. [Galina Churkina]	
3-451	A	10:5	10:16	This paragraph claims: "The few studies that have looked at hemispheric and global scales conclude that any urban-related effect is an order of magnitude smaller than decadal and longer timescale trends evident in the series (e.g., Jones et al., 1990)." This claim is untrue, for reasons argued in this and the next 2 rows. Jones (1990) should not be cited, and certainly not in support of the claim that it rules out a global imprint of urbanization effects on temperature data. First, the paper is 15 years old, and refers to data sets that are not the ones used in the AR4. Second, Jones 1990 only examines the US, the western USSR, eastern Australia and eastern China; hardly a global or even hemispheric sample. Third, it proves the opposite of the assertion being made. In the USSR data the trends are statistically insignificant in all series, including the combined urban-rural ones, but the post-1930 J-series (rural-urban) cools much less than the Rural series (by 0.11 oC) though this difference does not show up between the Rural and the V-series. In eastern China the urban warming (0.39 oC in only 30 years) is 2 to 3 times stronger than the combined rural-urban series (0.13-0.19 oC) and is almost twice as strong as a newly-constructed Rural series (0.23 oC). In the US, the urban-rural series (“grid”) grows by 0.15 oC more than the rural series, almost doubling the rate of rural warming. Yet in both the abstract and the conclusion of Jones et. al., they assert that their results provide little or no evidence of urbanization bias. They suggest that urbanization represents at most 0.05 oC of the observed 0.5 oC warming over the entire century. The 0.05 figure is not calculated anywhere in the paper, instead it represents a conjecture of the maximum that might be observed in key areas of the world they did not examine, i.e. Europe and the	

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				tropics (p. 172). Despite their own findings they assert that "In none of the three regions studied here is there any indication of significant urban influence" and "The United States result therefore does seem somewhat atypical compared with other industrialized regions of the world" (p. 172). This latter statement is especially odd since most of the other regions they look at do exhibit urban bias, and even if they didn't, eastern China, eastern Australia and the western USSR hardly constitute the "other industrialized regions of the world" outside the US. Jones (1990) provides support for the position that urban influences on temperature data do show up in many places, not that they are "an order of magnitude smaller". [Ross McKitrick]	
3-452	A	10:5	10:16	Parker (2005) is the only other support for the strong claims in this paragraph. Parker's paper is very short, relies on a visual comparison of trends and has only just appeared in print so it has not been subject to much critical discussion. The paper relies on the assumption that wind is a significant mitigating factor for the UHI. Yet Morris, Simmonds and Plummer (2000) showed that wind has only a small mitigating effect, that the effects diminish very rapidly (at the 1/4 exponential rate) and that cloud cover is more influential. These findings have been corroborated in several urban studies (see survey in McKendry 2003). Also Pielke and Matsui (2005) have argued that the Parker experiment is ill-posed, since there is no strong prior for assuming that the trend lines ought to be parallel under the null hypothesis of no urbanization bias. Overall these studies indicate that Parker's experiment is weakly-posed and confounded. It certainly cannot support the weight of conclusions being placed upon it. The UHI may simply be equally effective in both windy and calm subsamples; alternatively the UHI may be making equivalent two trend lines that would have otherwise differed. The papers of Peterson and Owens (2005) and Li et al (2004) are neither global nor hemispheric and are therefore not relevant to this paragraph. They may have bearing on the next paragraph and if so they should be cited therein. [Ross McKitrick]	
3-453	A	10:5	10:16	The claim that "the few studies" looking at global and hemispheric scales have found only minuscule effects is also false on the grounds presented in McKitrick and Michaels (2004). That study used a global sample, comparing effects observed in 218 weather stations with the effects in the corresponding grid cells. The authors established that there are statistically significant nonclimatic biases at the global level in the post-1979 trends from station data and that the same pattern carries over to the gridded data. They also showed that the biases likely add up to a net warming bias, accounting for one-third to one-half of the observed average trend. Please refer to cell G7 for a further discussion of this paper and its findings. [Ross McKitrick]	

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3-454	A	10:5	10:16	I suggest the paragraph be reworded as follows: "Many local studies have demonstrated that the microclimate within cities is, on average, warmer than if the city were not there, and that as cities grow or become more densely arranged the potential urban heat bias also grows. Adjustments are made to the data during the gridding process to try and remove these effects, with the aim of revealing an uncontaminated climatic signal. McKitrick and Michaels (2004) tested whether such adjustments are adequate to remove nonclimatic biases and concluded that they are very likely not. A significant pattern of effects attributable to local socioeconomic conditions is identifiable in raw station data and carries over to the gridded anomaly series. They concluded that the nonclimatic biases in the data add up to a net warming bias at the global level. In contrast, in a worldwide set of about 260 stations, Parker (2004) noted that warming trends in night minimum temperatures over 1950–2000 were not enhanced on calm nights, which would be the time most likely to be affected by enhanced urban warming. He argued the global land warming trend discussed is unlikely to be influenced by increasing urbanization. However, studies of conditions that support UHI development have shown that elevated wind speed is not a reliable mitigating factor, especially under cloudy conditions (Morris et al. 2000, McKendry 2003), nor is there reason to expect trends to be the same under varying wind conditions even without a UHI effect." [Ross McKitrick]	
3-455	A	10:5	10:16	Sources cited in this paragraph: McKitrick, R and P. J. Michaels (2004). “A Test of Corrections for Extraneous Signals in Gridded Surface Temperature Data” Climate Research 26(2) pp. 159-173. “Erratum,” Climate Research 27(3) 265—268; Morris, C.J.G., Simmonds, I. and Plummer, N. (2001): Quantification of the influences of wind and cloud on the nocturnal urban heat island of a large city. Journal of Applied Meteorology 40, 169–82; McKendry, Ian G. (2003). "Applied Climatology" Progress in Physical Geography 27(4) 597-606; Pielke, Roger A. Sr. and Toshihisa Matsui (2005). "Should Light Wind and Windy Nights Have The Same Temperature Trends At Individual Levels Even If The Boundary Layer Averaged Heat Content Change Is The Same?" Geophysical Research Letters, in press. [Ross McKitrick]	
3-456	A	10:6	10:6	A short discussion of UHI effect (city size, structure, season, climate) should be added before discussion of urbanization effect at larger scales [Galina Churkina]	
3-457	A	10:7	10:7	Insert 'large scale' before 'temporal trends'. [Marcia Baker]	
3-458	A	10:8	10:8	The comparison of an 'effect' with a 'trend' is not clear.	

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				[Marcia Baker]	
3-459	A	10:8		Replace "The few" with "Some" [Vincent Gray]	
3-460	A	10:9	10:10	I didn't exactly understand what was meant here. Does this mean that these 1% of sites were omitted in the Jones et al. series? Or that if they are omitted it doesn't make much difference? [Nathan Gillett]	
3-461	A	10:9		This detailed discussion of the various SST and MAT datasets seems excessive, especially given Appendix 3.A.3. All of this methodological detail should be eliminated unless it is needed to explain differences in results. The section should focus just on the results and include methodology only briefly, then only if it is new since the TAR. [Dian Seidel]	
3-462	A	10:11	10:14	Note that although on calms nights the UHI is stronger than in the other nights (so the differences between urban and rural sites increase), this does not mean that the mean urban minimum temperature in such nights has to be higher than the mean urban minimum temperatures in the other nights. [Javier Martin-Vide]	
3-463	A	10:11		Pielke Sr. has a publication now out which challenges some of the assumptions of Parker (2004) that should be pointed out. [John R Christy]	
3-464	A	10:13	10:14	"Thus" in line 13 to "(Parker 2004)." The statement is redundant [Vincent Gray]	
3-465	A	10:13		Add before "Over"; "However, McKitrick and Michaels (2004) have shown that the surface temperture records from 1979 to 2000 are significnatly influenced by a range of socioeconomic factors, such as income and GDP increase, coal usage, and datya quality. It is especially disturbing that dat from the former Soviet Union display multiple missing monthly figures between 1989 and 2001. [Vincent Gray]	
3-466	A	10:13		....unlikely to be influenced significantly by increasing urbanization .... [Jerry Mahlman]	
3-467	A	10:15	10:15	Please, consider modify a bit the quote between brackets as follow (Peterson, 2003; and Figure 3.2.3 from Peterson and Owen, 2005). Reference: Peterson, T.C., 2003: Assessment of urban versus rural in situ surface temperatures in the contiguous United States: no difference found. J. Climate, 16, 2941–2959. [Manola Brunet]	
3-468	A	10:16		Add at end "These studies do not eliminate the possibilty of urban effects in "rural"	

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				stations; below 30,000 population. [Vincent Gray]	
3-469	A	10:18	10:19	Again, please, consider adding to the clause between brackets the following: (i.e., as measured and transferred into the different datasets), as other mistakes with raw data can be incorporated when reformatting/transferring data from different datasets. [Manola Brunet]	
3-470	A	10:18	10:31	This assessment seems reasonable but do Kalnay and Cai still have a counterviewpoint? If so it should be stated and assessed. [Chris Folland]	
3-471	A	10:18	10:31	This is an awful lot of text for a non-result. Personally, I don't think the AR4 is obliged to spend a lot of space explaining why some studies are not as relevant as others, but I understand why others might disagree. I just suggest doing so in fewer words. [Dian Seidel]	
3-472	A	10:22		Rather than "Also" I think this sentence would be clearer if it started "But the reanalysis also did not include…". [Neville Nicholls]	
3-473	A	10:24	10:24	After (Trenbeth, 2004) add ".", delete "and" [Galina Churkina]	
3-474	A	10:25		Add at end " However, Peterson (2003) who claimed "No difference found" between" urban and rural stations in the USA from 1989-1991 actually did find a difference of about 0.15 C between the two sets. Also. Peterson(2003), Peterson et al (1998), Vose and Menne (2004) and Hansen et al (2001) have pointed out that the "homogeneity adjustments" necessary to obtain a reliable temperature record in the USA are impossible to make in most other parts of the world." [Vincent Gray]	
3-475	A	10:27	10:28	These comments do not appear to be adequately supported or born out by the Kalnay et al in press JGR manuscript. [Robert E. Dickinson]	
3-476	A	10:29	10:30	The reliability of reanalyses even since 1979 is highly debatable and will depend on the quantity examined (i.e., we know that it is not reliable for precipitation). All we know for sure is that they are definitely useless for looking at trends spanning the satellite and pre-satellite era. Suggest "Reanalyses may be reliable if confined to the period since 1979, but are definitely unsuited for periods prior to this as discussed..." [Steven Sherwood]	

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3-477	A	10:29		I'm misquoted here. At least insert "in general" after "Reanalyses" and "global" before trends. Simmons et al. (2004) in fact show for some regions reanalyses are able to capture trends quite well way before 1979. Over the North-American land mass, for example. [Adrian Simmons]	
3-478	A	10:35	10:37	should be "...had a detectable decrease in DTR..." There was in fact a decrease at all levels (Sherwood et al 2005) not detected in the cited study, though it was spurious. The excess of the detected increase at 850 relative to that at the surface is probably also spurious, so the second sentence here should be discarded. [Steven Sherwood]	
3-479	A	10:41	10:42	This doesn't sound like an example of land use change affecting DTR - rather it is just an example of DTR being different in different locations due to land cover. [Nathan Gillett]	
3-480	A	10:43	10:45	What is the sign and magnitude of the weekend effect? [Chris Folland]	
3-481	A	10:43	10:47	How does the Forster and Solomon paper (which does report an urban effect) align with earlier statements that urbanisation does not seem to be affecting trends? I think I understand this, but the placement of this sentence will lead a reader to ask this question. [Neville Nicholls]	
3-482	A	10:46	10:47	line 47, add reference Jin et al. (2005) at the end of the sentence "The weekly cycle …, most likely through changes in pollution and aerosols (Jin et al. 2005)". "Jin, M., J. M. Shepherd, M. D. King, 2005: Urban aerosols and their interaction with clouds and rainfall: A case study for New York and Houston. J. Geophysical Research, 110, D10S20, doi:10.1029/2004JD005081. This paper examines diurnal, weekly, seasonal, and interannual variations of urban aerosols. It proves that urban-induced aerosol has weekly cycle, which strongly support Forster and Solomon (2003) weekly temperature result. [Menglin Jin]	
3-483	A	10:47	10:47	Change "most likely" with "very likely" or virtually likely" (see Uncertainty Guidance Note). [Aristita Busuioc]	
3-484	A	11:0		Table 3.3 I would rather see the regions stacked together for direct comparison rather than categorized by data source, i.e. all NH, all SH etc. [John R Christy]	
3-485	A	11:1	11:7	There is a new paper recently submitted on satellite SST trends "The measurement of the sea surface temperature by satellites from 1991 to 2005". Anne G. O'Carroll, Roger W.	

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				Saunders, and James G. Watts. Accepted by J. Ocean and Atmosphere Technology. Its results deserve a mention though significant data gaps prevent a full assessment of the new (A) ATSR satellite SST trends. I think this gets referred to later in the chapter but should appear here. [Chris Folland]	
3-486	A	11:1	11:7	I think that this paragraph is a bit too optimistic. The satellites are hardly calibrated for physics-based repairs of serious "glitches" in satellite data. [Jerry Mahlman]	
3-487	A	11:7		Insert “the existence of” after “about” [Vincent Gray]	
3-488	A	11:9	11:9	This makes it sound a bit as though the IPCC actually did the research. [Nathan Gillett]	
3-489	A	11:12	11:16	How reliable is the method of using a GCM run? Does it depend on the GCM used? [Paolo Michele Ruti]	
3-490	A	11:24		add an apostrophe after "Reynolds" [Hartmut Grassl]	
3-491	A	11:30	11:37	I am struggling to understand what the second half of this paragraph is saying. Some clarification would be a help to readers. [Neville Nicholls]	
3-492	A	11:35	11:35	"Adapt" is a better word than "adopt". [Chris Folland]	
3-493	A	11:35	11:37	I assume they adapt rather than adopt. More seriously, it is difficult to prove that the new adjustments are correct so I would tone down this statement so that it is less damning of Smith and Reynolds - whose analysis may well be proven right in the longer term. [Peter Thorne]	
3-494	A	11:39	11:44	At this point or elsewhere in this context an explanation of the cooling trend c. 1880-1910 would be welcome (Fig. 3.2.4). [Christian-D. Schoenwiese]	
3-495	A	11:40	11:41	Mention that the major areas of data gaps in NMAT are still left blank in the optimally interpolated version. The technical NMAT description here and from line 46-40 needs to be integrated into continuous text. [Chris Folland]	
3-497	A	11:52		"less sampling" is ambiguous here. Should it be "averaging of fewer samples"? [Neville Nicholls]	
3-498	A	11:53	11:55	There was a paper by Folland and Karl or Karl and Folland in EOS around 2002 that looked at the possible disconnect between SST and NMAT trends that should be	

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				referenced. [John R Christy]	
3-499	A	11:55	11:55	Suggest adding the following reference: Diaz et al. (2005) J. Climate (submitted) Full ref. is: Diaz, H.F., R.J. Murnane, J.K. Eischeid, and A. Knap, 2005: Changes in air-sea temperature difference of the North Atlantic for the past 50 years. J. Climate (submitted). [Henry Diaz]	
3-500	A	12:13		"is STILL developing" or "is incomplete" might be better here. [Neville Nicholls]	
3-501	A	12:14	12:14	I encountered the acronym PDO and could not remember if it was previously defined [Steven Massie]	
3-502	A	12:16	12:21	The following new ref backs up this sentence: Knight, J., Allan R.J., Folland, C.K., Vellinga, M. and M.E. Mann, 2005: Natural Variations in the thermohaline circulation and future surface temperature. Geophys. Res. Lett., 32, L20708, doi: 1029/2005GL024233. The AMO may not be the full explanation of recent multidecadal variations of SST averaged over the North Atlantic. A contributory factor may be in phase variations of anthropogenic aerosol forcing in the last half century or more e.g. Baines, P.G. and C.K. Folland, 2005: Evidence for a rapid global climate shift across the late 1960s. Submitted to J. Climate. Discussion of phenomena like AMO etc in this section of the chapter is a very good innovation. [Chris Folland]	
3-503	A	12:19	12:19	Change “North Atlantic “ to”. North Atlantic Oscillation” [Aristita Busuioc]	
3-504	A	12:22		"Because of these inter-ocean differences" instead of "Accordingly"? [Neville Nicholls]	
3-505	A	12:27	12:30	Is there a reference to back this statement about the relative contribution of AMO and global warming or is this your assessment? [FILIPPO GIORGI]	
3-507	A	12:31	12:32	I prefer "…has now been replaced by strong warming…". [Neville Nicholls]	
3-508	A	12:31	12:32	It could be usefull a reference to figures 5.2.1 and 5.2.2 of chapter 5, considering the heat content of the ocean. [Paolo Michele Ruti]	
3-509	A	12:32	12:35	And just what are the arguments for "ALL OCEANS WARMED" in the early 1940's? I	

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				can only assume that this is a megatypo. [Jerry Mahlman]	
3-510	A	12:35		Delete "it is noteworthy how". Just saying it means it is noteworthy. [Neville Nicholls]	
3-511	A	12:41		It's not clear what is the GISS SST component and if it is independent from theUKMO. [Qiang Fu]	
3-512	A	12:44	12:44	The data of the Brohan et al (2005) paper should soon largely replace earlier data when the paper is accepted. It incorporates the new significantly enhanced ICOADS marine data in the combined land and ocean record for the first time (as well as lesser enhancements to Jones and Moberg (2003) land data) and extends the global surface temperature series back to 1850. It creates HadCRUT3 and HadCRUT3v [Chris Folland]	
3-513	A	12:46	12:46	Part of the text seems to be missing. [FILIPPO GIORGI]	
3-514	A	12:51	12:51	The Parker et al. ref is missing from the reference list. [Peter Thorne]	
3-515	A	12:55	12:55	Suggest adding "recent" before "warming is not yet quite". [Dian Seidel]	
3-516	A	12:55		Delete "conditions experienced" [Neville Nicholls]	
3-517	A	12:56	12:56	What "Patterns of Arctic warmth" are you referring to? Spatial, seasonal, interannual,…? [Dian Seidel]	
3-518	A	12:57		Is it worth (briefly) describing how the earlier conditions were different? [Neville Nicholls]	
3-519	A	13:0	13:	Table 3.3 The comments in cells G14-G17 apply equally to this Table as well. [Ross McKitrick]	
3-520	A	13:0		Many contradictions are in temperature trends, see table 3.2 et 3.3. Exemples : For 1901-2004 SH Land, CRU 0.073 SH SST, UKMO 0.068 and SH (CRU, UKMO) of table 3.3 is 0.061 smaller than the two hemisphere. NH (CRU,UKMO) 0.072 SH (CRU, UKMO) 0.061 and globe (CRU,UKMO) is 0.062. For 1910-1945 NH Land CRU 0.135 NH SST,UKMO 0.155 and NH (CRU,UKMO) of table 3.3 is 0.165 greater than NH Land et NH SST. The value of NH(CRU,UKMO) must be near 0.145. NH SST,UKMO 0.155	

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				SH SST,UKMO 0.140 the value of the Globe is 0.153 This value must be about 0.145 [Amar Matari]	
3-521	A	13:2	13:4	It is stated here that temperatures over Antarctica have increased, but are only weakly statistically significant over the whole period from 1958... Aside from not understanding the phrase "weakly statistically significant" (it is either statistically significant at some probability level or it is not), this discussion on Antarctic temperatures completely ignores the work by Peter Doran et al. (2002, Nature, 415, 517-520) which concludes that Climate models generally predict amplified warming in polar regions, as observed in Antarctica's peninsula region over the second half of the 20th century. Although previous reports suggest slight recent continental warming, our spatial analysis of Antarctic meteorological data demonstrates a net cooling on the Antarctic continent between 1966 and 2000, particularly during summer and autumn…Continental Antarctic cooling, especially the seasonality of cooling, poses challenges to models of climate and ecosystem change. The results of Doran et al. (2002) require that the original statement concerning Antarctic warming be tempered. [Jeffrey Kueter]	
3-523	A	13:3		What is "weakly statistically significant"? The increase? [Fons Baede]	
3-524	A	13:4	13:5	Warming over the Antarctic Peninsula since 1944 is arguably "very likely" [Chris Folland]	
3-525	A	13:4		Surely the Antarctic Penisular warming over the past 50 years is at least "very likely"? [Neville Nicholls]	
3-527	A	13:7	13:18	An omitted factor giving rise to the difference between land and ocean surface temperature changes is the contribution of latent heat of evaporation. While the heating effects of climate change will be uniformly distributed, the latent heat of evaporation will depress the temperature rise over ocean surface in comparison to that over land surface. The thermodynamics of radiative forcing remain consistent. The resultant increase in energy over land surface is reflected in higher temperatures, but the energy increase over ocean surface is reflected only partially by temperature rise, and partly by latent heat stored by the increased concentration of (non-condensed) atmospheric water vapour. [David Wasdell]	
3-528	A	13:7	15:48	Over the course of a few pages, at least three different sets of time periods are defined to distinguish different temperature trend behaviour. See specifically line 10 on page 13,	

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				line 9 on page 14, line 43 on page 15. Is it not possible to be more consistent? [Dian Seidel]	
3-529	A	13:8	13:18	Again some explanation/interpretation would be welcome, here with respect to the differences around 1890 and since c. 1985 (a reason for the latter discrepancy may be the different heat capacity of land and ocean). [Christian-D. Schoenwiese]	
3-530	A	13:12	13:16	The low frequency change of PDO, NAO and AO and the implication for attribution of climate change should be more emphasized. Warming since 1980s obviously occurred in the more positive NAO and AO condition… [Guoyu REN]	
3-531	A	13:16	13:18	Does the ozone depletion also contribute to this cooling? [Qiang Fu]	
3-532	A	13:16	13:18	Cooling also occurred in some areas of lands such as southwest China including the Sichuan Basin, southern Qinling Mts and southeastern Tibet (see Chen, L. X., Zhu, W. Q., 1998, Study on climate change of China over the past 45 years, Acta Meteorologica Sinica, 56 (3): 257-271; Ren, G. Y., Guo, J., Xu, M. Z., Chu, Z. Y., Zhang, L., Zou, X. K., Li, Q. X., Liu, X. N., 2005, Climate changes of Mainland China over the past half century, Acta Meteorologica Sinica, 63 (5) (in press in Chinese)) [Guoyu REN]	
3-533	A	13:17	13:19	Replace from "would be classed"' on line 18 to "chance " on line 19 with " which has not occurred since the 15th century (McIntyre and McKitrick 2003, 2005, Soon and Baliunas 2003 and Soon et al 2003) [Vincent Gray]	
3-534	A	13:20	13:26	It does not appear that the table uses italics for values in the 1-5% significance range, so there is no need to mention this in the legend. [Dian Seidel]	
3-535	A	13:25		comment: I see no italics! [Hartmut Grassl]	
3-536	A	13:29		Fig. 3.2.7: I like the idea of splitting into tropics and far extratropics, but how many boxes/stations is the trend SH65S on based on? Is this a good point to split? [Gabriele Hegerl]	
3-538	A	13:33		Fig. 3.2.9: trend 1901-2004: This has much more coverage than my plot of the 20th century trends, even though mine is based on loosening the coverage criterion quite a bit beyond my comfort zone. Mine has no data in large part of interior of Africa, Asia and the Pacific ocean. I worry that your criterion allows the trend to be based on a shorter time than the 20th century, which may be a bit misleading...	

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				[Gabriele Hegerl]	
3-540	A	14:0	17:	the period is mentionned from 1901-2004 instead of 2005 [Matari Amar]	
3-541	A	14:0	26:	the value 0,03K seem small as an error of the whole 25 years of observed data, [Matari Amar]	
3-542	A	14:0		This is a very nice analysis, and serves as the measurement anchor for much that is written in this chapter. However, it should be noted that we have to be very carefull about interpreting regional temperature "changes" as long-term regional trends. Clearly, we have to be very careful in our descriptive word choices, since we do have regional examples of what appear to be trends(say, mid-latitude continental continental warming and drying). [Jerry Mahlman]	
3-543	A	14:1	14:25	(as previous) The difference in how trends in temperature are being expressed in this assessment relative to the TAR is a key issue that will attract a great deal of attention. It would probably be helpful to many readers if the authors would explicitly state that the trend when computed in the same manner as in TAR remains 0.6 C over the 20th century. The information is there but I think a more explicit statement is needed. [Susan Solomon]	
3-544	A	14:2	14:5	This sentence is awkward. It mentions 'two consecutive years' and then mentions two sets of consecutive years. It's unclear what years the difference of 0.27K is based on. [Dian Seidel]	
3-545	A	14:3	14:5	Fig. 3.2.6 not illustrated as a, b, and c. Lines refer to Fig. 3.2.6a. Also, values are supposed to represent calendar year means, but text refers to 1956/1957 and 1963/1964 using the following wording: greatest difference between two consecutive years. Should rephrase to indicate that it is referring to averages using two consecutive years. [Henry Diaz]	
3-546	A	14:4	14:4	0.27 K between 1956/1957 and 1963/1964, --> 0.27 K (1956/1957 and 1963/1964), [Koji Yamazaki]	
3-547	A	14:8	14:8	Since the linear trend is a poor approximation, it would be worth to make trend estimates through other models, for example a "sloped steps" model. [Franco Desiato]	
3-548	A	14:8	14:14	After presenting a rationale for splitting the postwar period at 1979, this paragraph moves the split to 1971, for no apparent reason. The t statistic referred to in line 10 is not referenced to a published source, and should not be used in this report.A "simple t statistic" is not the appropriate test for a difference of means across autocorrelated samples.	

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				[Ross McKitrick]	
3-549	A	14:8		a full stop is missing after "years" [Hartmut Grassl]	
3-551	A	14:9	3:9	Verify that 2005 is correct there. Shouldn't be 2004? [Enric Aguilar]	
3-552	A	14:9	14:10	Some readers may be confused by the use of "latter". Why not say "was warmer than 1936 to 1970 which in turn was warmer than 1901-1935? And use consistent ranges, either 1901-1935 or 1901 to 1935, but not both in the same sentence. [Neville Nicholls]	
3-553	A	14:10		The T test makes a comparaison between twoo means, so a new test which compares the three means simultaneously is better, [Matari Amar]	
3-554	A	14:10		the t test makes a comparison between two means , so a new test can be mentioned which compares the three means simultaneously . [Amar Matari]	
3-555	A	14:12	14:14	The sentence beginning "Splitting" is badly written. "….the only time at which each decade was warmer than the preceding one was the 1950s and 1960s." would be a better way of finishing it. [Adrian Simmons]	
3-556	A	14:16	14:18	The statement is not correct formulated: instead of "global average surface temperature linear trend has very likely warmed…extremely unlikely…." should be "global average surface temperature has very likely increased by 0.6….exceptionally unlikely….". [Aristita Busuioc]	
3-557	A	14:16	14:17	"the global average surface temperature linear trend has very likely warmed by just over 0.6 +/- 0.2 K". In this case confidence limits are given (presumbaly 5-95%, since 'very likely' is used). However, the use of 'just over' makes this more difficult to interpret. [Nathan Gillett]	
3-558	A	14:16	14:29	This conclusion "from this section" is better to move before section 3.3 [ILEANA MARES]	
3-559	A	14:16	14:29	We have not evaluated the effect of urbanization on surface air temperature records globally. Some regional analyses indicate significant effect of urbanization or land use change. If it is large on global scale, the conclusion drawn in this paragraph would be modified. [Guoyu REN]	
3-560	A	14:16	16:29	This paragraph has redundancies. In line 20 "a fairly linear upward trend" since 1970 is about the same as the concept in line 27 that "the global trends over the past 30 years has	

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				been fairly linear". [Dian Seidel]	
3-561	A	14:17	14:17	Verify that 2005 is correct there. Shouldn't be 2004? [Enric Aguilar]	
3-562	A	14:17	14:17	Really 2005 or more correct 2004? [Christian-D. Schoenwiese]	
3-563	A	14:17		the period is mentioned from 1901 to 2004 instead of 2005 [Amar Matari]	
3-564	A	14:17		You don't use "just over 0.6" when you transfer this statement to the Executive Summary (see my comment #1) [Neville Nicholls]	
3-565	A	14:18		a comma is missing after "(Chapter 6)" [Hartmut Grassl]	
3-566	A	14:19	14:21	This characterization of the global temperature history is misleading. You cannot ignore the temperature rise prior to 1920 (arguably beginning around 1905 or so) and yet include the rise prior to 1977. There is no compelling reason to either include or not include either period, however, you should not included one and ignore the other. By suggesting that the most recent warming started in 1970, you are contradicting other portions of the chapter (e.g. page 6, lines 13-16) where the argument is made that the recent temperature rise began after 1976. [Jeffrey Kueter]	
3-567	A	14:20	14:21	Don't' know what Figure QACC3.1 is. [Henry Diaz]	
3-568	A	14:21	13:21	Warming of 0.75C since when? [Chris Folland]	
3-569	A	14:21	14:22	The end points should not be filtered! [Rasmus E. Benestad]	
3-570	A	14:21	14:22	I think care needs to be taken quoting the trend from low-pass filtered data as 0.75K. It is clear how the linear trend is calculated: a least squares fit is made to the annual mean data, and the gradient is multiplied by the period considered. However, the 'nonlinear' trend could be calculated in any number of ways, and the method used will affect the answer. To take an extreme example, one might derive the 1901-2005 trend by differencing the means of the years 2005 and 1901. Alternatively, one might difference the first and last 5-yr, 10-yr, or 20-yr means. Each would give a different answer, and it's not clear which is the best estimate to use. If this figure of 0.75K warming becomes one of the key results of the report, I think more discussion and justification needs to be given	

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				for the particular method used to calculate it. I would favour concentrating on the linear trend, since its derivation is clear. [Nathan Gillett]	
3-571	A	14:21	14:22	Clearly using a low-pass filter is biasing the warming towards the latter period. The linear trend through the temperatures from 1977-2004 (a period of 28 years) is 0.0171ºC/year leading to a total rise during that period of 0.48ºC. The trend for the period 1918 through 1945 (also 28 years) is 0.0155ºC/year producing a total rise of 0.434ºC. The observed temperature rises are nearly equal, but the low-pass filter indicates that the rise is twice as much during the latter period. Thus it misrepresents the true behavior of the temperature changes since 1900. The results from the low-pass filter should be deleted from the text. [Jeffrey Kueter]	
3-572	A	14:21	14:22	It is very problematic to refer trends to low-pass filtered data because in this case the trend values depend on the filter type and, moreover, filtered data may contain fluctuations influencing the trend values. Therefore only linear (or non-linear) trends should be specified: this holds for the whole chapter. [Christian-D. Schoenwiese]	
3-573	A	14:21		insert a space between "QACCS" and "3.1" [Hartmut Grassl]	
3-574	A	14:23	14:23	Change “warm” with “increase”. [Aristita Busuioc]	
3-575	A	14:26		the value 0.03 k seem small as an error on the whole 25 years of observed data [Amar Matari]	
3-576	A	14:27	14:27	The statement is similar with those presented at line 20 , same page.; I suggest to delete it. [Aristita Busuioc]	
3-577	A	14:28		Expressions such as “over the last 5 years” should be written out explicitly in years such as “2000-5” to avoid being anachronistic and ambiguous once the report is published (in a few years time!). [David Stephenson]	
3-578	A	14:31		Section 3.2.2.7: The bulk of this section discusses DTR changes. However, DTR changes have little relevance over the ocean (which is not discussed) and would be better combined with the earlier discussion of globally averaged DTR changes in Section 3.2.2.1 (land-surface only) or at least in a part of that section that deals with land surface changes on a regional basis. A global mean perspective of change is relatively meaningless without an appreciation of the contributing regional changes, and these aspects should be more closely linked than they are currently.	

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				[John Caesar]	
3-579	A	14:32	14:47	Authors should poiht out the substantial cooling trend for 1979-2004 for much of Canada, and offer an explanation. It appears to be largest swath of continental cooling in globe. [Henry Diaz]	
3-580	A	14:32	14:47	Prefer Figs 3.2.9 and 3.2.10 to all have the same scale (as in TAR). Makes comparisons easier. [Chris Folland]	
3-581	A	14:34	14:38	Besides, cooling also occurred in southwest and central China including the Sichuan Basin, southern Qinling Mts and southeastern Tibet (see Chen, L. X., Zhu, W. Q., 1998, Study on climate change of China over the past 45 years, Acta Meteorologica Sinica, 56 (3): 257-271; Ren, G. Y., Guo, J., Xu, M. Z., Chu, Z. Y., Zhang, L., Zou, X. K., Li, Q. X., Liu, X. N., 2005, Climate changes of Mainland China over the past half century, Acta Meteorologica Sinica, 63 (5) (in press in Chinese)) [Guoyu REN]	
3-582	A	14:36	14:36	According to Figure 3.2.9, replace 'and a part of Bolivia' by 'and parts of Bolivia and Zaire' [Javier Martin-Vide]	
3-583	A	14:39	14:39	I suggest to add 'and Southeastern Brazil.' after 'ocean regions of the SH' [Javier Martin-Vide]	
3-584	A	14:51		Insert "or reanalysis" after "model" [Adrian Simmons]	
3-585	A	14:54	14:54	Is there any explanation available for this NH-SH contrast? Ocean-dominated SH data should have a better representativeness so that I wonder that the SH error bars are larger. [Christian-D. Schoenwiese]	
3-586	A	15:0		Give the warming by decade because the lenght of the periode not the same 1920-1940 and 1970-2004 [Matari Amar]	
3-587	A	15:0		Question 3.1 Although it is useful to describe the warming of global temperatures over the last 120 years or so as several periods of warming and cooling rather than an overall warming, it can give the false impression that the climate change has gone through distinct transitions. The exact choices of trends described (and shown in Question 3.1, Figure 1 pp 165) are arbitary, and give the impression that something within the climate "switched on or off" at those points. In reality the processes of internal climate variability and externally forced factors are likely to have gone through gradual changes which combine to give the overall climate change.	

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				I suggest that the definition of the periods examined is made more "fuzzy" and vague. [Gareth S. Jones]	
3-588	A	15:0		Give the warming by decade because the lenght of the periode is not the same, 1920-1940 and 1970-2004. [Amar Matari]	
3-589	A	15:1	15:16	Please add the following "minority statement" (or is it perhaps a majority?): In general DTR is a magnitude extremely sensitive for inhomogeneities (more than mean temperature). Therefore there is a strong need to invest further work in data quality improvement especially for DTR before going into detailed studies on effects that might be artefacts of non climatic effects. One such effects is happening right now: automation of temperature measurement which often goes along with a change from Stevenson screens (lower DTR) versus smaller, ventilated screens (higher DTR) and has significant influence on DTR. [Reinhard Böhm]	
3-590	A	15:1	15:16	Please, add the following results among the regional-scale results: In Italy DTR has significantly increased over the last 50 years in agreement with a marked cloud cover decrease (Maugeri et al. (2001); however, the sign of the trend is negative if the last 150 years are considered (Brunetti et al., 2005). REFERENCES: 1) M. Brunetti, M. Maugeri, F. Monti, T. Nanni. 2005. Temperature and precipitation variability in Italy in the last two centuries from homogenized instrumental time series. International Journal of Climatology, in press.; 2) M. Maugeri, Z. Bagnati, M. Brunetti, T. Nanni. 2001. Trends in Italian total cloud amount, 1951-1996. Geophys. Res. Let., 28, 4551-4554. [Michele BRUNETTI]	
3-591	A	15:1	15:25	Given the recent lower importance of DTR changes, I would like to see this section shortened. [Chris Folland]	
3-592	A	15:1	15:16	comment: too much text for such a short period of time [Hartmut Grassl]	
3-593	A	15:1	15:16	I would suggest to highlight that in Italy a significant change in the long-term evolution of the DTR has been observed in the 1950s. As a consequence the sign of the trend - that is negative at secular scale - becomes positive if only about the last 50 years are considered (Brunetti et al., 2005). REFERENCES: Brunetti, M., Maugeri, M., Monti, F., Nanni T., 2005: Temperature and precipitation variability in Italy in the last two centuries from homogenised instrumental time series. Int. J. Climatol., in press.	

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				Maugeri, M., Bagnati, Z., Brunetti, M., Nanni, T., 2001: Trends in Italian total cloud amount, 1951-1996, Geophys. Res. Lett., 28, 4551-4554. [Teresa NANNI]	
3-594	A	15:1	15:25	DTR decrease was said as an evidence of enhanced greenhouse effect in SAR and TAR. Now it tends to be stable for some places, and the past change and the recent stability are being explained by cloud cover and precipitation. AR4 should give an explaination for the evolution of thinkings... [Guoyu REN]	
3-595	A	15:1	15:16	This paragraph seems to present new results and thinking about DTR trends, that they are not generally negative in most places. You'd never know there was a new result from the first sentence of the paragraph, though. It should be re-worked to alert the reader of the new findings. [Dian Seidel]	
3-596	A	15:2	15:3	change "DJF and season" in " DJF season". [Aristita Busuioc]	
3-597	A	15:2	15:3	Missing a season after 'and' in "..largest in the DJF and season (not shown)." [John Caesar]	
3-598	A	15:2	15:3	"trends tend to be largest in the DJF and season" : "trends tend to be largest in the DJF season" (?) [Jean-Marc Moisselin]	
3-599	A	15:3	15:3	Remove "and". [Qiang Fu]	
3-600	A	15:3		"and season"? Probably some text missing. [Fons Baede]	
3-601	A	15:3		extra "and" or missing season [Steven Sherwood]	
3-602	A	15:5	15:6	So which result is correct? Dai et al or the results reported here? And why the inconsistency? [Neville Nicholls]	
3-603	A	15:8	15:8	Please, consider adding this sentence or a similar one after (Tuomenvirta et al., 2000) outlining findings found over Spain in a recent study of daily maximum and minimum temperatures evolution during the 1850-2003 period: ", but in disagreement with findings of a recent study (Brunet et al. 2005) over mainland Spain for the period 1850-2003, which show larger increases of annual and seasonal trends of maximum temperatures than minimum temperatures both when analysing the 22 individual records and the corresponding regional curves."	

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				Reference: Brunet et al. 2005. The development of a new dataset of Spanish daily adjusted temperature series (SDATS) (1850-2003). International Journal of Climatology, submitted. [Manola Brunet]	
3-604	A	15:11	:13	That DTR has not changed over India is not clear from fig 3.2.11. On the contrary! [Fons Baede]	
3-605	A	15:14	15:14	Replace "historic" with "historical". [Aiguo Dai]	
3-606	A	15:27	15:39	Move this entire section to page 3_32 line 44, so that all the information on global tempoerature change is together [Vincent Gray]	
3-607	A	15:29	16:39	Inserting the answer to question 3.1 here interrupts the flow as the troposphere has not been discussed in depth at this point. Also, the tropospheric (which level?) satellite trends seem to be from one data set and the tenor of the treatment seems to somewhat biased towards all problems being solved. They are not. The treatment does not reflect the substantial structural uncertainty in satellite record trends, let alone the radiosonde record, so needs revision to be consistent with the current text of US CCSP Report No1. [Chris Folland]	
3-608	A	15:29	16:39	In Question 3.1. The temperature is given in Celsius degree and in the previous text the temperature is given in Kelvin, except for Table 3.1., and this might be a problem for a nonspecialist. [ILEANA MARES]	
3-609	A	15:29		Question 3.1: In the answer to Question 3.1 reference is made to upper-air temperatures although these are dealt with only much later in para 3.4. Place this Question after para 3.4 or perhaps place all Questions at the end of the chapter [Fons Baede]	
3-610	A	15:31	15:48	Probably should make a statement about a temperature decrease between 1940 and 1970: the answer provided suggests that there was an increase of 0.85 deg C, whereas Table 3.3 ~0.62 (Fig. 3.2.1 suggests similar temperatures in 1900 and 1920...). Presumably there was a temperature decrease of ~ -0.23? Below is says a cooling of 0.1deg C (0.75 deg C warming in total?). This is a bit confusing. Also seems repetitive. [Rasmus E. Benestad]	
3-611	A	15:31	15:35	You cannot ignore the temperature rise prior to 1920 (arguably beginning around 1905 or so) and yet include the rise prior to 1977. There is no compelling reason to either include or not include either period, however, you should not included one and ignore the other. By suggesting that the most recent warming started in 1970, you are contradicting other portions of the chapter (e.g. page 6, lines 13-16) where the argument is made that the	

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				recent temperature rise began after 1976. The linear trend through the temperatures from 1977-2004 (a period of 28 years) is 0.0171ºC/year leading to a total rise during that period of 0.48ºC. The trend for the period 1918 through 1945 (also 28 years) is 0.0155ºC/year producing a total rise of 0.434ºC. The observed temperature rises are nearly equal, but the low-pass filter indicates that the rise is twice as much during the latter period. Thus it misrepresents the true behavior of the temperature changes since 1900. It is unfair the suggest the early century warming ran from 1920 to 1940, when clearly an convincing argument can be made that it extended from about 1907 to 1945—a period nearly twice as long! Further, it is stated, on page 3-06, lines 13-16, "The 1976 divide is the date of a widely acknowledged 'climate shift' and seems to mark a time when global mean temperatures began a discernible upward trend that has been attributed to increases in greenhouse gas concentrations in the atmosphere." Taking 1970 as the starting date and truncating the warming period of the early 20th century is misleading because it exaggerates the magnitude of the late-20th century warming, compared to that early in the century. [Jeffrey Kueter]	
3-612	A	15:31	15:55	Is there now compelling evidence that the "repairs" of the dysfunctional "climate-monitoring" satellites have now been (finally!) repaired? If so, are there now open-literature references that validate this assertion? I have recently been informed that the satellite diagnostic teams have now united to try to "cure" their blatently pathological problems that have caused so much confusion. Quite frankly, I am still unconvinced that these deeply serious problems have now been "fixed". [Jerry Mahlman]	
3-613	A	15:31	15:35	This material is a bit misleading regarding the surface and tropospheric temperature changes since 1958. The overall change may be similar, but the time evolution is rather different, with most of the tropospheric warming occuring abruptly in the mid 1970s and the surface showing clearer upward trends in the most recent decades. [Dian Seidel]	
3-614	A	15:31		"Important variations regionally". Where is the detailed information about this and why does it not appear or be considered in Chapter 11?. [Vincent Gray]	
3-615	A	15:31	:33	The reporting is a bit selective, listing only the periods with warming (adding up to more than the total observed!) - if there is cooling inbetween, should be listed also even if mentioning small/insignificant [Gabriele Hegerl]	
3-616	A	15:33	15:35	Am I missing something? Since 1979, the radiosonde, reanalyses and UAH satellite temperatures since 1979 show a warming total of about +0.32 K, not +0.55 K as implied	

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				(perhaps need to separate the starting points of 1979 and 1970). Thus, the statement here is not backed up by the evidence presented in this chapter and is very misleading. Indeed, when statistical significance of the difference time series is applied, the differences are significant. (Standard errors based on temporal statistical representation answer a different question than what is needed here.) There is more I would like to contribute regarding my own work in North Alabama (Christy 2002 BAMS), California (Christy et al. 2006 JClimate in press) and soon to be submitted Kenya, which indicate local surface trends are likely overstated a bit by the global networks now being promoted. However, you don't have much room to get down into the weeds on this. I'll pass. [John R Christy]	
3-617	A	15:33	15:34	I think it is clear that the lower atmosphere is "a distinctly differet region", so you don't need to say this. [Neville Nicholls]	
3-618	A	15:33	15:35	Wang et al. shows that temperature change has been smaller in the middle and lower layers of troposphere over China since 1960, and the rate of change is only 0.05C/10 years. Warming has mainly occurred in winter (January), while other seasons has shown slight cooling trend. Annual mean surface air temperature change as calculated based on radiosonde station surface data in 1961-2004 reaches 0.30C/10a, however, which is an order of magnitude higher than that in the middle and lower layers of troposphere in the same period (please see Wang, Y. and Ren, G. Y., 2005, Change in free atmospheric temperature over China during 1961-2004, Climate and Environmental Research, 10 (4) (in press in Chinese)). Many other researchers also obtained much smaller change in upper air temperature globally or regionally (please see Angel (1999), NRC (2000), Mears, et al. (2003), Christy and Norris (2004) et al.) [Guoyu REN]	
3-619	A	15:33	15:35	This is a mis-representation of the state of the science. The result for radiosondes over the longer period is mainly fortuitous as a result of fitting a linear trend diagnostic to two timeseries of very different character. It also implies that the satellite era is no longer a "problem", but there is still ambiguity over whether the troposphere is warming or cooling relative to the surface over this period. Regardless of the semantics above, if this is an integral part of the answer then the question is ill-posed because the troposphere is above the earth and not on the earth. Also, the use of standard errors to assess consistency is just wrong. We can't play "do the error bars overlap?" and hope to be taken seriously, as I argue elsewhere in my review. Uncertainty in linear trend fits is definitively the wrong diagnostic to use. Uncertainty in the difference series is the correct diagnostic once corrected for auto-correlation effects and will tend to highlight residual problems. [Peter Thorne]	

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3-620	A	15:34		Replace "similar" with "somewhat lower" and insert "but" before "within" [Vincent Gray]	
3-621	A	15:38	15:38	What is Question 3.1, Figure 1? Is it Fig. 3.2.1? [Henry Diaz]	
3-622	A	15:39	15:39	Where does the value "2000 stations" come from? On page 3-8, line 33, the numbers in each of the three surface dataset considered are given as 4167, 5985, and 6308. Is the dataset used here different than either of these three as previously described? [Jeffrey Kueter]	
3-623	A	15:42	15:46	text similar with those presented at page 14, lines 19-25 [Aristita Busuioc]	
3-624	A	15:42	15:44	You cannot ignore the temperature rise prior to 1920 (arguably beginning around 1905 or so) and yet include the rise prior to 1977. There is no compelling reason to either include or not include either period, however, you should not included one and ignore the other. By suggesting that the most recent warming started in 1970, you are contradicting other portions of the chapter (e.g. page 6, lines 13-16) where the argument is made that the recent temperature rise began after 1976. The linear trend through the temperatures from 1977-2004 (a period of 28 years) is 0.0171ºC/year leading to a total rise during that period of 0.48ºC. The trend for the period 1918 through 1945 (also 28 years) is 0.0155ºC/year producing a total rise of 0.434ºC. The observed temperature rises are nearly equal, but the low-pass filter indicates that the rise is twice as much during the latter period. Thus it misrepresents the true behavior of the temperature changes since 1900. It is unfair the suggest the early century warming ran from 1920 to 1940, when clearly an convincing argument can be made that it extended from about 1907 to 1945—a period nearly twice as long! Further, it is stated, on page 3-06, lines 13-16, "The 1976 divide is the date of a widely acknowledged 'climate shift' and seems to mark a time when global mean temperatures began a discernible upward trend that has been attributed to increases in greenhouse gas concentrations in the atmosphere." Taking 1970 as the starting date and truncating the warming period of the early 20th century is misleading because it exaggerates the magnitude of the late-20th century warming, compared to that early in the century. [Jeffrey Kueter]	
3-625	A	15:44	15:44	"Figure QACCS3.1" is not understandable for a normal reader [Reinhard Böhm]	
3-626	A	15:44	15:44	What is Figure QACCS3.1? [Henry Diaz]	
3-627	A	15:50	15:55	Out of place? Should be moved to p. 14 L 48? [Rasmus E. Benestad]	

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3-628	A	15:50	15:55	The paragraph should be expanded to give details of changes in individual regions, including a Table. And actual figures [Vincent Gray]	
3-629	A	15:54		Prefer "a reduction in the number" rather than "fewer". [Neville Nicholls]	
3-630	A	16:1	16:3	The oversimplification of land use impacts on regional and global scales is repeated here. Dr. Roger Pielke, Sr. is a leading scientist in the field of the impacts of land use change on the local, regional, and global climate, yet, none of his research on the topic is included in this section. Dr. Pielke suggests the following in regards to how land use changes and their impacts be included in the IPCC AR4: As recognized by the National Research Council in 2005 (Radiative Forcing of Climate Change: Expanding the Concept and Addressing Uncertainties) land-use/land-cover change is a first-order climate forcing. However, its role as a regional and global climate influence is not widely recognized, except as it effects the atmospheric concentration of carbon dioxide and the global average surface albedo. In the summary figure from the IPCC TAR Statement for Policymakers, in terms of the global mean radiative forcing, only albedo effects of land use/land cover change are identified. However, numerous studies have shown that the effect of land-cover/land-use change is to alter temperatures and precipitation in regions where the change occurs, as well as weather globally through teleconnections (see, for example, Pielke, R.A., et al., 2002, Phil. Trans. R. Soc. Lond. A, 360, 1705-1719, and Marland G., et al., 2003, Clim. Pol., 3, 149-157). We should, therefore expect global climate effects from land-use/land-cover change. The next IPCC needs to focus more on this first-order climate forcing than they have in the past. The question of searching for a “discernable effect on the climate system” misses the obvious in that we have been altering regional and global climate by land-use/land-cover change for decades. The issue of land use change is so important that it should be afforded its own subsection under Section 3.2, rather than as an afterthought in subsection 3.2.2.2 (Urban temperature and urban heat island). [Jeffrey Kueter]	
3-631	A	16:1	16:6	This paragraph needs to be revised after correcting the discussion as explained above in cells G19-G22. [Ross McKitrick]	
3-632	A	16:1	16:6	Some investigators obtained different results, and those should be taken into account and a more balanced evaluation should be given. We have also investigated into the effect of	

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				urbanization on area-averaged surface air temperature trend as obtained from RB stations in north China, for example, and the result shows that the effect of urbanization on observed increase in anuual mean surface air temperature of national reference and basic stations in North China is very obvious, reaching 0.11C/10a for 1960-2000 period, accounting for 38% of the total warming recorded.(please see Zhou, Y. Q. and Ren, G. Y., 2005, Identifying and Correcting Urban Bias for Regional Surface Air Temperature Series of North China over Period of 1961-2000, Climate and Environmental Research, 10 (3) (in press in Chinese)). Others include Hansen and co-authors, 2001; Hughes and Balling, 1996; Portman, 1993; Zhou, et al., 2004; Kalnay and Cai, 2004; Chu and Ren, 2005..... [Guoyu REN]	
3-633	A	16:1		Replace “A number of” with Some” [Vincent Gray]	
3-634	A	16:4	16:6	Delete sentence about weekend effects - not closely relevant to this question. [Neville Nicholls]	
3-635	A	16:4	16:26	Also suggest defining or omitting the following terms: urban heat island effects ln 4; teleconnection effects, ln 10; radiosonde, ln 23 [David & David Wratt & Fahey]	
3-636	A	16:5	16:5	Q3.1: The acronym "DTR" might not be familiar to readers of the questions. How about expanding it out to "diurnal temperature range"? [David & David Wratt & Fahey]	
3-637	A	16:6		Replace “A number of” with Some” [Vincent Gray]	
3-638	A	16:6		I suggest replacing "alleviation of" by "lower" [Adrian Simmons]	
3-639	A	16:8	16:12	I would start this paragrap with the sentence starting "In only a few regions…". I think the previous four sentences are obvious and not really needed here. [Neville Nicholls]	
3-640	A	16:9	16:9	Insert “types of “ between “The two” and “extremes”. [Rasmus E. Benestad]	
3-641	A	16:14		There is considerable spatial variability…" instead of "Spatial variability is important… [Neville Nicholls]	
3-642	A	16:18	16:20	Would you please, consider due reference to support these statements: "Since 1950, the lehgth of frost-free season has increased in most mid--to-high latitude regions ….in the autumn". [CONSTANTA-EMILIA BORONEANT]	
3-643	A	16:22	16:39	As above	

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				[John R Christy]	
3-644	A	16:22	16:32	Suggestion: "Global temperatures aloft can be estimated from satellite data since 1979 and from the radiosonde network since 1958. Although the tropospheric temperatures need not be the same as surface temperatures, the two are expected to show related trends on long timescales. Satellite estimates provide global coverage, but have been taken from 13 different instruments since 1979, and cross-calibration of those instruments to create a homogeneous record introduces uncertainties in long-term changes. After accounting for the influence of the stratosphere on satellite measurements of the troposphere, lower tropospheric trends ..." [Melissa Free]	
3-645	A	16:22	16:39	As for p. 3, lines 41-51 above, could be put even more strongly.; but the details given later are excellent. [Robert KANDEL]	
3-646	A	16:22	16:39	See also my point 1. This section seems to have glossed over problems with the tropospheric temperature records. Also, why is it acceptable to quote radiosonde trends when they support the prevalent message (1958-onwards) but then to reject them when they disagree (1979-onwards). This seems to be rather inconsistent, and for the "sceptic" is surely evidence that the observations are being subjectively selected to bolster one particular viewpoint. [Mark McCarthy]	
3-647	A	16:22	16:39	This is a very brief account of the vertical profile of temperture trends in the atmosphere. A more thorough treatment comes later in the chapter, and so I wonder why such a cursory treatment is given here. Here at least should be a summary of major advances since the TAR, which include the development of more two homogenized radiosonde datasets and two additional MSU datasets and some important advances in understanding the nature of the uncertainty in all the observations and resulting time series. Also, the phrase (line 32) "After all the necessary adjustments have been made" is extremely misleading. Most people involved in this area of research would shudder at the thought that we have identified all the problems with the data, let alone have made all the necessary adjustments. Also, the sentence (line 36-39) on the consistency between surface and tropospheric trends is misleading, as mentioned in comment 19. [Dian Seidel]	
3-648	A	16:22		There is VTPR data before 1979 [Adrian Simmons]	
3-649	A	16:23	16:24	I would consider the layer between 850-300 hPa or 2-8 km as the mid-troposphere or troposphere instead of "lower troposphere".	

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				[Qiang Fu]	
3-650	A	16:23		Why 1958 for radiosondes? The southern hemisphere network was indeed enhanced for the IGY, but it does not have a good enough oceanic coverage to enable reliable global temperature averages to be calculated. Conversely, radiosonde coverage in the northern hemisphere was quite good for the northern hemisphere from the late1940s onwards due to the ocean weather ships that provided marine coverage prior to the satellite era. Some rewriting of the sentence is needed. [Adrian Simmons]	
3-651	A	16:25	16:25	tropospheric measures have not to follow the same evolution than surface temp… [Bernard Seguin]	
3-652	A	16:26	16:26	The meaning of the acronym MSU could be given in brackets after the acronym, ie "… MSU (Microwave Sounding Unit) …". [David & David Wratt & Fahey]	
3-653	A	16:30		Insert "difference" before "between the sensors." [Neville Nicholls]	
3-654	A	16:32	16:32	remove "lower". [Qiang Fu]	
3-655	A	16:32	16:36	This is a mis-use of statistics. What do standard errors have to do with consistency? They describe how well the data fit a pre-conceived notion of a linear change. How can this be used to define the consistency of datasets where much of the high frequency varaiance is common, or put another way the r^2 is high? To do this requires use of a difference series to remove the pooled variance. If you want to assess consistency then the correct timeseries is a difference series. The text as written almost appears to be a lazy attempt to justify saying that everything is fine and dandy when the reality is different. I am happy with a statement to the effect that warming aloft may be equal to or greater than that seen at the surface, but am unhappy with text that either implicitly or explicitly excludes the alternative. This does not characterise the state of the science which is that the relative sign of surface / troposphere trends is unknown. This is an advance from the previous position whereby the troposphere was believed to be cooling relative to the surface, so not something to be "ashamed of". [Peter Thorne]	
3-656	A	16:33		Replace “similar” with “somewhat lower” [Vincent Gray]	
3-657	A	16:33		this statement is true only for the global mean. [Steven Sherwood]	
3-658	A	16:34		Substitute for "Evidence" the following text: "Recent inferences from newly corrected	

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				satellite data" [Melissa Free]	
3-659	A	16:36	16:38	"Estimates from radiosondes since 1958 also agree with surface estimates, but the radiosonde network has markedly poorer coverage than the surface network and is not global in extent. " Radiosonde network has advantages over the surface network in some aspects, and the surface network also has some problems which could not properly be solved. [Guoyu REN]	
3-660	A	16:36	16:38	The assertion that surface and sondes agree is wrong. The timeseries exhibit very different behaviour on decadal timescales, but exhibit similar high-frequency variability and (likely by chance) trends. Also, the implied argument that you need similar sampling density at the surface and aloft is false. Planetary waves act to smooth out anomalies aloft such that many fewer stations are required to adequately sample large-scale variability. In the tropics you only need about 3 to 5 equally spaced samples (assuming perfect data recording of course) to characterise tropic-wide changes. Whereas at the surface (relatively speaking) many more points are required. [Peter Thorne]	
3-661	A	16:36		Suggest substituting for rest of paragraph from "Estimates" the following text: "The radiosonde network allows greater vertical resolution and a longer period of record, but has markedly poorer coverage than the surface network and pervasive homogeneity problems that impede assessment of long-term changes. Estimates from adjusted radiosonde data since 1958 show greater warming in the troposphere than at the surface, but trends since 1979 show similar or less warming in the troposphere than at the surface in the tropics. Some new evidence suggests that this lack of recent warming is due to remaining biases in the radiosonde record. In sum, despite large uncertainties, the record of temperature changes aloft from satellites and radiosondes is not inconsistent with observed changes at the surface. [Melissa Free]	
3-662	A	16:45	24:56	This entire section was quite 'dense' with more attention to methodologies and detail than in other sections. I would recommend a summarising down of the methods with far more focus on results and summarising material. [M James Salinger]	
3-663	A	16:49	16:50	This sentence is awkward. Please rewrite it. [FILIPPO GIORGI]	
3-664	A	16:49	16:54	This is a very poor introduction and virtually repeated in several places elsewhere. It is, however, important information. As it is alluded to in many places it may be worth collating into a box that is entitled "The importance of a multi-variate approach" which	

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				outlines that temperature is a necessary, but not an adequate constraint to describe the evolution of the climate system. And then proceeds to outline the range of other variables and how we can use these to better constrain our understanding. The arguement is already in the chapter but is spread over so many places that the message is likely to be lost, particularly to the non-specialist. A rationalisation would greatly benefit the chapter. [Peter Thorne]	
3-665	A	16:51	16:52	Here, in contrast to p4, lines 18-22--see #9 above--the importance of the role of the radiative dynamic in driving trends in ET is explicitly (if clumsily) stated: Radiative forcing (and this predominantly solar radiation) alters heating, and at the Earth's surface this directly affects evaporation as well as sensible heating. So any discussion of ET trends should take place in the context of trends in solar radiation (e.g., using Penman/Priestley-Taylor/complementary relationship), not just of temperature-forcing (Thornthwaite). In recognition of the fact that it is radiative forcing that directly supplies the energy for evaporation, this sentence should more clearly read "At the Earth's surface, radiative forcing directly affects evapotranspiration and sensible heating." [Michael Hobbins]	
3-667	A	16:52	16:55	This is not a new result. It is simply the Clausius-Claperon equation of chemical physics. [Jerry Mahlman]	
3-668	A	16:55	16:57	This sentence implies that were it not for the influence of aerosols, an upward trend in precipitation would have been observed. I think our uncertainties in precipitation response to greenhouse gas forcing are also very large, and relate to opposing effects of the surface warming, and the increased optical depth of the troposphere (Allen and Ingram, 2002). For example, some CMIP models show a decrease in precipitaiton in response to GHG increases (Raisanen et al., 2002, J. Clim., 15, 2395-2411). Ziegler et al. (2003, J. Clim., 16, 535-547) use a perfect model study to demonstrate that an anthropogenic response in precipitation is not likely to be detectable at present. [Nathan Gillett]	
3-669	A	16:56	16:56	This effect of aerosols is not at all certain. Insert the word 'may' before 'act'. [Marcia Baker]	
3-670	A	16:56	16:56	"which act to short circuit the hydrological cycle" could be explained more clearly. [Franco Desiato]	
3-671	A	16:56	16:56	It is not clear that aerosols short-circuit the hydrological cycle. In one sense they are critical catalysts in allowing water droplets to form readily, but act as capacitors through their scarcity as ice nuclei and through their over-abundance in some polluted stratiforn; as well as other roles – except short-circuit. [Michael Manton]	
3-672	A	16:56		What does "short circuit" mean here? Change to clearer terminology.	

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				[Neville Nicholls]	
3-673	A	16:57		Insert "expected" before "effect"? [Neville Nicholls]	
3-674	A	17:0	17:0	Somewhere in the document it could be helpful to discuss whether there is a change in the form of precipitation from snow to rain in some regions. This could be a topic in chapters 3 or in 4, but I hope the authors will discuss that and consider putting it somewhere. [Susan Solomon]	
3-675	A	17:0		In my mind it should be emphasised that changes in precipitation are very much site-specific (subchapter 3.3.2). The 5x5 degree grid reflects the variety of changes in precipitation only in continental scale but not at all in smaller scale. Trends may be different at rather close stations. For example, in Norway in the Scandinavian Mountains, a significant increase in precipitation is detected on western slopes and western sides of the mountains and not in the eastern slopes. It lies in a good concordance with the increase in intensity of westerlies (NAO index). During the short period (1979-2004) trends in precipitation may be influenced by random extremely high 2-3 day summer rainfalls, which were observed in Estonia in 2003 in Johvi and 2004 in Tallinn, for example. It means that short-term trends in precipitation at some stations are very random and not representative for larger areas. [Jaak Jaagus]	
3-676	A	17:1	17:2	This sentence is awkward. Please rewrite it. [FILIPPO GIORGI]	
3-678	A	17:4	17:13	Suggest a couple or so references on precipitation measurement problem from raingauges at this point, as well as cross referral to the relevant Appendix.. [Chris Folland]	
3-679	A	17:4	17:13	A/the major problem of precip over the ocean is how to get an unbiased estimate, which is vital for climate change purposes. [Michael Manton]	
3-680	A	17:4		The difficulties in monitoring a variable such as precipitation, which has large variability in both space and time, could be mentioned here instead than at line 49, same page. [Annarita Mariotti]	
3-681	A	17:5	17:6	evaporation effect could be added to illustarte the fact that problems are different for different regions. [Jean-Marc Moisselin]	
3-682	A	17:7		The greatest problem with remote data is the relatively short record, restricting ability to calculate trends [Neville Nicholls]	

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3-683	A	17:9	17:13	This sentence is awkward. Please rewrite it. [FILIPPO GIORGI]	
3-684	A	17:15		This section should probably contain a brief discussion of precipitation trends in mountainous regions. Indeed, several studies find that the (relative) precipitation trends in mountainous regions are stronger than those in the surrounding foreland. For the Alps, a nice demonstration of this is provided by Schmidli et al. (2002). [Christoph Schar]	
3-685	A	17:17	17:16	I would suggest eliminating the global average precipitation trend estimates for periods prior to 1979 altogether. They are presented without critique assessment of the data availability throughout the century, without assessment of biases related to instrumentation changes and the elevation of the stations used and their changes during the 20th century. The land precipitation time series presented are not "global" by any means and infilling used for all data sets (except one, GHCN) masks this situation and misleads the readers. The only possibility that I see would be to present the map with part of the global land that has century-long time series, clearly identify it and present the time series for it (all data sets listed could be used here but the GHCN would be the best, because it guarantees the reader against the misuse. Precipitation is not temperature, its changes are much less smooth spatially and presentation of a regional average (let's say, 50% of the land area), while being justified for temperature, is completely unacceptable for precipitation as global representative totals. Precipitation gridding is misleading in this regard. "We" can say that "we" are well aware of this. But, when from time to time "we" are looking at global coverage of precipitation maps that are published throughout the journals, finally, e+G18ven those who initially understood all caveats of this presentation lose their discipline. An example is on page 18 line 21, where "we" state as a sure thing about especially high precipitation increases over high latitude regions in Canada during the 104 years. In these regions the observations started after World War II and acute data homogeneity problems should be addressed prior to any discussion. Sometimes, we hear that "gridding is "better than nothing" but it well can be "worse than nothing" because it (a) creates a false impression of data availability, (b) suppresses to some extent the real data gathering (why to care, let's say, about China or Canadian data problems and try to clean them when we already have "everything" in nice and convenient grid format), and (c) misleads the users who take the gridded data without any ability to verify their input and "far downstream" generates results and practical implications that could be dangerously wrong. Bottom line: I suggest not putting the IPCC stamp on questionable global statements and carefully specifying the regions where we can say anything about precipitation changes during the past century. Also, the 25 year long period (since 1979) should be presented but words of caution should be said about short time series used to present the global picture of the latest changes.	

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				[Pavel Groisman]	
3-686	A	17:17		There is too much information about individual precipitation datasets that is not necessary for explaining what, if any, new findings they have revealed. In general I think the AR4 should stress scientific results, not dataset construction issues. [Dian Seidel]	
3-687	A	17:18	17:28	Intrinsic global annual rainfall uncertainties must be very great, especially in earlier decades, as shown by the considerable disagreements in the modern period. If likely annual uncertainties were included in the Diggle calculations, would the trend of 0.98% per decade still be significant? [Chris Folland]	
3-688	A	17:18	17:28	Although it is a custom to integrate up precipitation into annual global land precipitation anomalies this does not have a lot of meaning to me. I would prefer to see precipitation split into significant circulation belts or areas which would then provide more diagnostic information on the changes that are occurring. [M James Salinger]	
3-689	A	17:18	17:28	This paragraph presents the upward trend since 1901 without mentioning until several paragraphs later that trends appear to be downward sinde 1951. This is poor organization of information and misleading if someone does not read carefully. The anomalies for individual years can be given later. [Steven Sherwood]	
3-690	A	17:20	17:28	Figure 3.3.1 looks more like a regime change in the 1950s than a linear trend over 100 years. [Michael Manton]	
3-691	A	17:21	17:21	Firstly, what do you mean be “statistically significant”? One should always give a significance level (eg 5%). However looking at Fig 3.3.1, I'm not convinced it means a lot. If there is a trend, it's certainly not linear, as the more recent part appears to contain sub-intervals with negative trends (especially early 1970s to mid 1990s). You could use a students t-test to test the difference in the average levels of the early and recent periods, but it almost appears that the variance has increased as well as the mean level. This could be investigated with a wavelet analysis. [Rasmus E. Benestad]	
3-692	A	17:21	17:21	Increase only until roughly 1955 so that the time history of this series should be described more exactly (slight decrease since c. 1955, very pronounced fluctuations superimposed). [Christian-D. Schoenwiese]	
3-693	A	17:21		If the discrepancies in trend are substantiel it is better do not utilize the terme significant in line 21 of page 17	

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				[Matari Amar]	
3-694	A	17:22	17:28	There are inconsistencies between the text and the mentioned figure (figure 3.3.1). The following corrections (see cells below) are necessary to solve these inconsistencies, but the problem could be due to a wrong version of the figure (as indicated in one of the following comments) suggested by the inconsistency between the figure caption and the title of the figure box. [Michele BRUNETTI]	
3-695	A	17:22	17:23	In 2003 the negative anomaly does not reach –0.90% (-10mm), this value was reached in 2001. [Michele BRUNETTI]	
3-696	A	17:22	17:24	"-10 mm" here is inconsistent with Fig. 3.3.1. [Qiang Fu]	
3-697	A	17:22	17:28	There are inconsistencies between the text and the mentioned figure (figure 3.3.1). They are probably due to a wrong version of the figure, as there is also inconsistency between the figure caption and the title of the figure. [Teresa NANNI]	
3-698	A	17:22	17:28	Delete details of anomalies in recent years - not very relevant to trends, or even interesting [Neville Nicholls]	
3-699	A	17:23	17:24	The sentence "This was a larger departure from normal than in 2002" must probably be changed in "This was a larger departure from normal than in 2003". [Michele BRUNETTI]	
3-700	A	17:25	17:25	2001-2004 is not a period of negative annual global precipitation anomalies, being 2002 and 2004 positive, and only 2001 and 2003 negative anomalies. [Michele BRUNETTI]	
3-701	A	17:25	17:27	Looking at figure 3.3.1 global lend areas had NOT been dominated by a period of positive anomalies in the period here indicated (from 1990s to 2000). [Michele BRUNETTI]	
3-702	A	17:30	17:31	change "…data sets covering more recent periods and…" to "…data sets covering more recent periods, one (CRU, Mitchell and Jones, 2005) covering the entire 20th century, and…" [Reinhard Böhm]	
3-703	A	17:37	17:41	The section dealing with the two long term GPCC data sets is somewhat mistakable. Things would maybe clarified by replacing this passage with: „The Global Precipitation Climatology Centre (GPCC) provides two monthly data sets from surface gauges on 0.5 , 1.0 and 2.5 grids. Both data sets have been constructed using several GPCC sources	

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				(including data from CRU, GHCN and FAO and many nationally data sets provided from more than 170 nations). While the data set designated GPCC VASClimO (Beck et al. 2005) uses only quasi-continous station time series that have been subjected to homogeneity testing and homogenisation and as well detection and elimination of outliers in order to improve temporal homogeneity the GPCC Full Data set (updated from Rudolf et al. 1994) uses all available stations to provide more complete spatial coverage in each individual month." Reference for Beck et al. 2005 – Beck, C., J. Grieser and B. Rudolf (2005): A new monthly Precipitation Climatology for the global land areas for the period 1951 to 2000. Climate Status Report, 2004: 181-190, German Meteorological Service – available via http://www.dwd.de/de/FundE/Klima/KLIS/prod/KSB/ksb04/28_precipitation.pdf [Christoph Beck]	
3-704	A	17:37	17:43	The naming and description of the 2 GPCC products is incorrect. As a member of the GPCC I suggest to replace these lines by "The Global Precipitation Climatology Centre (GPCC) provides 2 long term monthly data sets from gauges on 0.5, 1.0 and 2.5 grid. Both, the GPCC-Full Data Product and the GPCC-VASClimO Climatology are based on a combination of the CRU, GHCN, and FAO database as well as datasets provided by more than 170 nations. The Full Data Product is interpolated as in Rudolf et al. (1994) and uses all data available. The VASClimO Climatology (Beck et al., 2005) uses only long and homogenious data series of about 10,000 stations to interpolate relative deviations from the long term averages based on the observations from 29,000 stations. A local kriging interpolation is used in the latter case." [Jürgen Grieser]	
3-705	A	17:45		Looking at Fig.3.3.1, it is not surprising that no significant trends are found (and in fact results often have opposite signs). The "spread" of the results should not however be interpreted as an indicator of the poor quality of the datasets. In addition, the period over which the trends are calculated varies by one or year or more, this could also contribute to the differences. [Annarita Mariotti]	
3-706	A	17:46	17:47	I cant see how "scatter and uncertainties" undermine the result of statistical significance. Significance tests take account of the scatter. [Neville Nicholls]	
3-707	A	17:53	18:13	This table shows tremendous scatter and inconsistent signals among the different data sets. Changes in precipitation are best illustrated by Fig. 3.3.2--top panel for the period 1901-2004. [Henry Diaz]	
3-708	A	18:0		Table 3.4. After PREC/L 19482002 should be changed to 1948-2002	

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				[Enric Aguilar]	
3-709	A	18:0		Why the standard error is very greater for 1979-2004 (more stations) than 1901-2004, See table [Matari Amar]	
3-710	A	18:0		Table 3.4.a. column 1, row 5 – GPCC VASClimO instead of GPCC [Christoph Beck]	
3-711	A	18:0		Table 3.4.a. column 1, row 6 – GPCC Full Data instead of GPCC V.3 [Christoph Beck]	
3-712	A	18:0		Table 3.4.a. column 2, row 5 – 1951 -2000 instead of 1948 - 2002 [Christoph Beck]	
3-713	A	18:0		Table 3.4.a. column 2, row 6 – 1951 -2004 instead of 1951 – 2002 [Christoph Beck]	
3-714	A	18:0		Table 3.4.a. column 6, row 5 – Reference to Beck et al., 2005 instead of Rudolf et al. 1994 [Christoph Beck]	
3-715	A	18:0		Reference for Beck et al. 2005 - Beck, C., J. Grieser and B. Rudolf (2005): A new monthly Precipitation Climatology for the global land areas for the period 1951 to 2000. Climate Status Report, 2004: 181-190, German Meteorological Service – available via http://www.dwd.de/de/FundE/Klima/KLIS/prod/KSB/ksb04/28_precipitation.pdf [Christoph Beck]	
3-716	A	18:0		Table 3.4.b. column 1, row 5 – GPCC VASClimO instead of GPCC [Christoph Beck]	
3-717	A	18:0		Table 3.4.b. column 1, row 6 – GPCC Full Data instead of GPCC V.3 [Christoph Beck]	
3-718	A	18:0		Table 3.4.b. column 4, row 5 – Superscript b should be deleted, as GPCC Full Data series proceeds until 2004 [Christoph Beck]	
3-719	A	18:0		Table 3.4.b. column 4, row 6 – Superscript b should be deleted, as GPCC Full Data series proceeds until 2004 [Christoph Beck]	
3-720	A	18:0		Table 3.4. First row of PREC/L series: replace 19482002 by 1948-2002 [Manola Brunet]	
3-721	A	18:0		Table 3.4a: second line: "1948-2002" instead of "19482002" [Jürgen Grieser]	
3-722	A	18:0		Table 3.4a: 5th line: "VASClimO" instead of GPCC, "1951-2000" instead of "1948-2002" and "Beck et al., 2005" instead of "Rudolf et al., 1994"	

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				[Jürgen Grieser]	
3-723	A	18:0		Table 3.4a: 6th line: "GPCC-Full" instead of "GPCC V.3", "1951-2004" instead of "1951-2002", and "Rudolf et al., 1994" instead of "Rudolf et al., 1998" [Jürgen Grieser]	
3-724	A	18:0		Table 3.4.a : The first period of record must be separated 1948-2002 [ILEANA MARES]	
3-725	A	18:0		it quite surprising, given the results which are being shown, that no explicit mention is made of the annual and seasonal trends over the Southern Europe/Mediterranean/Northern Africa region, with trends quite comparable to other regions which are discussed more extensively. it is especially so given the projection for change in these regions. [Annarita Mariotti]	
3-726	A	18:0		Why the standard error is very greater for 1979-2004 than 1901-2004? If the discrepancies in trends are substantiel, it is preferable do not utilize the term significant in line 21 of page 3-17. [Amar Matari]	
3-727	A	18:0		Section 3.3.2.2: considering the strong decadal variability shown in figure 3.3.1, it could be usefull to comment on the reliability of the spatial patterns of the precipitation trend. [Paolo Michele Ruti]	
3-728	A	18:0		Table 3.4, second column, PREC/L: Write 1948-2002 (dash missing). [Christian-D. Schoenwiese]	
3-729	A	18:1		a hyphen is missing between "19482002" (column 2, line 1) [Hartmut Grassl]	
3-730	A	18:1		correct: "GPCC V" and "GPCP V" to "GPCC v" and "GPCP v" resp. (column 1) [Hartmut Grassl]	
3-731	A	18:6	18:6	There is such a wide range for each of the time periods in the table that some discussion is necessary. Also, with these uncertainties, present values with only one significant figure. [Marcia Baker]	
3-732	A	18:11	18:13	The assumption of limited spatial sampling by in situ gauge data adding temporal noise is unconvincing and may affect not only GPCC data sets but also the other gauge based data sets. Moreover systematic biases due to satellite data are not relevant for the two GPCC data sets as none of them use satellite data. Generally speaking - in my opinion the discussion of the assumed quality of data sets is misplaced in the context of a note referring to a table. [Christoph Beck]	
3-733	A	18:11	:13	Table 3.4b: (Notes) - "This suggests …." Drop this sentence. Do not start explanations and argumentation in a table legend. GPCC data can not be afflicted by the life time of	

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				satellites because satellite data aren't used at all. [Jürgen Grieser]	
3-734	A	18:13		correct: "GPCC" to "GPCP" [Hartmut Grassl]	
3-735	A	18:17	19:9	Selectively mention zonal average trends; show numerical values in Fig 3.3.2 and especially 3.3.3 (much as the display method used in the TAR). [Chris Folland]	
3-736	A	18:18	18:19	This sentence is awkward. Please rewrite it. [FILIPPO GIORGI]	
3-737	A	18:18	18:35	The description of map (Figure 3.3.2) is inadequate at least. Focus on the regions where the data do exist! High latitude regions in Canada, Amazon basin, West Africa are just examples where flaws are most evident. Is precipitation at one station (Manaus, I suppose) enough to claim wet conditions over the Amazon Basin? Are there enough century long precipitation time series for West Africa? I worked with Mexican and Australian rainfall data and do not believe that these countries can be covered in their entirety by sufficiently dense network with century-long rainfall time series. These questions should be answered first to us G5and then balanced statements should be included in the section. The relaxed condition (66 of 104 years with valid data) used in Figure 3.3.2 allows for some data points to start from the late 1930s (instead of 1901) that could create a large mess in northern extratropics where the 1930s were a special period. For e+G5xample, years of dust bowl in the US and Canada, warm Arctic (with temperature similar to present conditions), and droughts in Central Eurasia all happened in this decade. G17 [Pavel Groisman]	
3-738	A	18:20	18:20	Here it would be good to explain why the report is relying on GHCN as a dataset given the others available. [Dian Seidel]	
3-739	A	18:21	18:35	No significant long-term change in the country-averaged annual precipitation was seen for both the past 50 years. However, an obvious tendency of drying in the Yellow River Basin and the North China Plain in terms of precipitation has been found, and the largest drop in precipitation occurred in Shandong Province. The area with a marked decrease trend of annual precipitation also includes southern Northeast China and northern Korea Penisula actually. Meanwhile, an insignificant wetting trend in the Yangtze Basin and most parts of western China could be detectable. For the Yangtze Basin, the increased annual precipitation mainly resulted from the significant rising of summer rainfall, though winter precipitation also tended to increase (Ren, G. Y., Guo, J., Xu, M. Z., Chu, Z. Y., Zhang, L., Zou, X. K., Li, Q. X., Liu, X. N., 2005, Climate changes of Mainland China	

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				over the past half century, Acta Meteorologica Sinica, 63 (5) (in press in Chinese)). [Guoyu REN]	
3-740	A	18:24	18:24	change "…Amazon Basin and southern…" to "…Amazon Basin and southeastern…" [Reinhard Böhm]	
3-741	A	18:28	18:31	I suggest to group the two sentences as following: After having concluded that the effect of changing rainfall gauge networks on sahel rainfall time series is small, Dai et al (2004) note that Sahel rainfall in 1990 has recovered.. [Bernard Seguin]	
3-742	A	18:31	:33	Fig 3.3.2 does not show an increase (1901-2004) or decrease (1979-2004) in precip over eastern India. I suppose that the text is meant to refer to north-westerly India? [Fons Baede]	
3-743	A	18:33	18:34	In fact the spatial and temporal variation of rainfall over Australia for the last century was quite complex. [Michael Manton]	
3-744	A	18:34		Include "areas with" before "increases". [Neville Nicholls]	
3-745	A	18:39	18:39	Figure 3.3.3 has more data gaps for 1979-2004 than for 1901-2004, at least for Australia and New Guinea. [Michael Manton]	
3-746	A	18:41	19:2	This section made no sense to me even after several read throughs. I have no suggestions as I find it entirely impenetrable text, but it obviously requires revisiting. [Peter Thorne]	
3-747	A	19:0		This snowfall change analysis is interesting and credible. Also, we have suspected that a warming planet is very likely to INCREASE the Antarcticsnowfall, and thus the preciptiation over the Antarctic continent. It also appears to be consistent with the tightening of the Antarctic Annular Mode, as described later in this Chapter Page 22. [Jerry Mahlman]	
3-748	A	19:0		Section 3.3.2.3: this sub-section needs for a short synthesis at the end. [Paolo Michele Ruti]	
3-749	A	19:1	19:2	Are the precipitation trend spatial patterns meaningful by noting large differences between datasets in Table 3.4? [Qiang Fu]	
3-750	A	19:2	19:3	This attribution is not necessarily true and in any case is not really appropriate for this chapter. [Steven Sherwood]	
3-751	A	19:3	19:4	According to the figure, the decreasing trend in southern	

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				Africa is in JJA, not DJF. Likewise, the Indian changes are reversed. However, the descriptions for Chile and Australia appear to be correct. [Steven Sherwood]	
3-752	A	19:7	19:9	This sentence is awkward. Please rewrite it. [FILIPPO GIORGI]	
3-753	A	19:8	19:9	replace the sentence "Much like the annual…..during this period" by: "A striking seasonal feature in Europe is the sea-saw like pattern of recent increase/decrease of SON/DJF-precipitation in Central Europe (Brunetti et al., 2005) and an opposite evolution in Fennoscandia." [Reinhard Böhm]	
3-754	A	19:9	19:9	Text about Eurasia is missing here but resurfaces in the next section (3.3.2.3.Changes in snowfall). Some text reallocation will be needed (e.g., from line 20, of this page). [Pavel Groisman]	
3-755	A	19:10		To model spatia patterns of precipitation trends, what is needed is a model, that would allow for complex patterns of changes in precipitation events. For instance, the model should allow for (a) characteristics of a space-time stochastic process or a random field, (b) changes not only in the average precipitation amounts (as is understood by a classical trend function), also the whole probability distribution function (pdf) may change, (c) changes in extremes while the average may remain the same so that typical trend analysis would not detect changes. Parsimonious modeling of a stochastic process that would accommodate such characteristics is in Ghosh et al. (1997) who consider the temporal case, and extended to space & time in Draghicescu (2002). Nonparametric quantile surface smoothing offers one method for assessing various patterns of changes in precipitation trends including precipitation extremes. [Sucharita Ghosh]	
3-756	A	19:11	19:18	There seems to be some disagreements between the described behaviour of precipitation over Canada (an increasing trend) in this section and what is stated in the next pages (from line 55-page 23 to line 3 page 24) as a justification of the decreasing streamflow. Both discussions are supported by some citations from the literature. The problem is that, in the first case, the cited work (Stone et al., 2000) is referred to an analysis period that stops in 1995, while, in the second case, Dary and Wood (2005) refer to a more updated data set. This consideration is confirmed also by other works: Rood et al. (2005) agree with Dary and Wood (2005) (both used updated data sets), while Przybylak (2002) agrees with Stone et al. (2000) (in these cases both data sets are updated to 1995). REFERENCES: 1) Rood, S.B., Samuelson, G.M., Weber, J.K., Wywrot, K.A., 2005, Twentieth-century decline in streamflows from the hydrographic apex of North America,	

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				Journal of Hydrology, vol. 306, p. 219-233; 2) Przybylak, R., 2002, Variability of total and solid precipitation in the Canadian Arctic from 1950 to 1995, Volume 22, Issue 4, 30 March 2002, Pages 395-420. [Michele BRUNETTI]	
3-757	A	19:11	19:55	Here or earlier mention the problem of artificial increasing trends in precip from raingauges as snow changes to rain (Forland and Hansen-Bauer, 2000 in the ref list). [Chris Folland]	
3-758	A	19:11	19:55	Precipitation includes snowfall. And is it necessary to separate snowfall from rainfall? [Guoyu REN]	
3-759	A	19:12	19:12	replace "Figure 3.3.2" by "Figure 3.3.3" [Reinhard Böhm]	
3-760	A	19:12	19:28	The whole paragraph is not very clear [Franco Desiato]	
3-761	A	19:12	19:12	Reference to Fig. 3.3.2--should be to Fig. 3.3.3, which illustrates seasonal change patterns [Henry Diaz]	
3-762	A	19:12	19:12	Its Fig 3.3.3. [Chris Folland]	
3-763	A	19:12	19:14	Fig. 3.3.2 shows annual PRCP, not winter PRCP. [Christian-D. Schoenwiese]	
3-764	A	19:12	:28	How reliable are showfall data? [Gabriele Hegerl]	
3-765	A	19:21		I don't think you need "on a circumpolar basis". [Neville Nicholls]	
3-766	A	19:28		"…falling AS SNOW in winter…"? Or is this obvious? [Neville Nicholls]	
3-768	A	19:30		The results describing regional trend of snowfall in North America, suggest adding results for another part of North America: "Berger et al. (202) found that there was a trend toward fewer snowfall events during the winter seasons across the lower Missouri river basin over the period 1948 - 2002. This was especially true during the 1980's and 1990's, and during the spring season. At the same time, there was little or no trend in snowfall occurrences within the plains region to the south of the Berger et al. (2002) study as found by Lupo et al. (2005). Berger et al. (2002) also show that this region of the country experienced fewer snowfall events during strong El Nino events, and these results are consistent with those of Kunkel and Angel (1999) and Smith and O'Brien (2001), who also reported less snowfall during El Nino winters over the same areas. These studies also show that the results above were found to be related to the cyclone variability (occurrence	

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				and type) over the region. || Berger et al. (2002): A climatology of northwest Missouri snowfall events: Long-term trends and interannual variability. Phys. Geog., 23, 427 - 448. || Kunkel, K.E. and J.R. Angel, 1999: Relationship of ENSO to snowfall and related cyclone activity in the contiguous United states. J. Geophys. Res., 104, 19425 - 19434. || Lupo, A.R. et la. (2005): Interannual variability of snowfall events in southwest Missouri and Liquid-toSnowfall Water ?Equivalents at the Springfield WFO. Nat Wea. Dig., 29, in press. || Smith, S.R., and J.J. O'Brien, 2001: Regional snowfall distributions associated with ENSO: Implications for seasonal forecasting. Bull. Amer. Meteor. Soc., 82, 1179 - 1191. [Anthony Lupo]	
3-769	A	19:41	19:55	Several different snow and ice characteristics (snowmelt, ice mass, snow depth) are discussed in this paragraph, while only the snowfall changes should be described here. Please, coordinate with Chapter 4 and correct when needed. [Pavel Groisman]	
3-770	A	19:46		After "…-1999", add: "Scherer et al. (2004) have shown that in the late 20th century decreases in snow days in the swiss Alps have been observed for stations below 1300 m asl and that his decrease can mainly be attributed to an increase in temperature." [Scherrer SC, Appenzeller C, Laternser M, 2004: Trends in Swiss Alpine snow days: The role of local- and large-scale climate variability. Geophys. Res. Letts. 31 (13)] [Martine Rebetez]	
3-771	A	19:54	19:55	Sentence starting "It is suggested that this…" is not relevant to this chapter. [Neville Nicholls]	
3-772	A	19:54	19:55	The sentence starting "It is suggested…" needs a reference or should be dropped. [Dian Seidel]	
3-773	A	20:0		I was surprised, that no mention of results from re-analyses are being mentioned here. Analyses of moisture flux variability, more directly linked to observations than precipitation, have been very useful in the study of oceanic precipitation. Also, despite the differences in climatological values, the variability in the satellite based datasets and re-analyses are often in good agreement, especially on interannual timescales. [Annarita Mariotti]	
3-774	A	20:1	20:37	Although it has been criticised, I guess you should mention Rosenfeld's suggestion that urban pollution may decrease rainfall. [Michael Manton]	
3-775	A	20:1	20:37	What is the implications of the urban precipitation change? But urban-induced temperature change is more relevant to the global climate change detection, and more pages should be given to it. [Guoyu REN]	

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3-776	A	20:1	57:20	This whole discussion is very qualitative. What is the conclusion? [Marcia Baker]	
3-777	A	20:1		3.3.2.4 paragraph is interesting, but does not treat the consequences on the possible bias for global temp trends [Bernard Seguin]	
3-778	A	20:2	20:4	This sentence is awkward. Please rewrite it. [FILIPPO GIORGI]	
3-779	A	20:16	20:19	This little sentence about land use changes in the middle of the section on urban effects reads as a minor aside. In fact, land use changes like deforestation could be much more important than urbanization because of the land area involved. Either treat the issue more carefully, or omit it from this discusion. [Dian Seidel]	
3-780	A	20:18	20:18	Really? Decreasing precipitation in deforestation areas would be more plausible because of reduced transpiration. [Christian-D. Schoenwiese]	
3-781	A	20:21	20:29	I think this paragraph can be deleted entirely. [Neville Nicholls]	
3-782	A	20:32		delete: "being" [Hartmut Grassl]	
3-783	A	20:35	20:37	Delete last 2 sentences of this paragraph - not really relevant to this chapter, or very interesting. [Neville Nicholls]	
3-784	A	20:39	21:43	Suggest section 3.3.2.5 is roughly halved in length given the general lack of clear results. [Chris Folland]	
3-785	A	20:39	21:43	This section to me (very much not an expert) reads a bit long given results [Gabriele Hegerl]	
3-786	A	20:40	20:40	Before satellites, there was no existing observations? Is it true? [Bernard Seguin]	
3-787	A	20:41	20:43	Please clarify, this sentence is quite criptical. [Annarita Mariotti]	
3-788	A	20:44	20:48	This sentence is awkward. Please rewrite it. [FILIPPO GIORGI]	
3-789	A	20:50	3:51	This statement requires a reference, in my opinion. [Neville Nicholls]	
3-790	A	20:57		Change "deemed" to "currently" [Adrian Simmons]	

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3-791	A	21:6	21:6	Replace 'each other' with 'themselves' [Marcia Baker]	
3-792	A	21:14	21:14	What is a hydrometeor? [Nathan Gillett]	
3-793	A	21:15	21:17	Some mention of cutoff in TRMM PR should go here. [Marcia Baker]	
3-794	A	21:17	21:17	What exactly is 2A25? Is this a reference that hasn't been picked up? Or an algorithm? If the latter a reference may help. [Peter Thorne]	
3-795	A	21:19		The oceanic precipitation changes associated to the NAO have not being mentioned, in the North Atlantic these are relevant to the ongoing discussion about the freshening of the North Atlantic. Related studies are Bojariu and Reverdin, 2002 and Mariotti and Arkin, 2005, in the process of being submitted. [Annarita Mariotti]	
3-796	A	21:28	21:29	English is not clear. [Marcia Baker]	
3-797	A	21:29		"everywhere" rather than "regionally". [Neville Nicholls]	
3-798	A	21:38		"decreases" instead of "negative change values". [Neville Nicholls]	
3-799	A	21:39	21:40	"This pattern of changes in the tropics is similar to a combination of El Nino and La Nina patterns". Surely to first order these patterns cancel out. Does this mean that the response looks like the nonlinear component of the ENSO response? Or that in some areas it looks like El Nino and in other areas la Nina? If it's the former, more clarification is needed. If it's the latter, then it sounds like the response doesn't look like ENSO at all. [Nathan Gillett]	
3-800	A	21:39	21:40	I don't understand sentence "This pattern of changes in tropics…". [Neville Nicholls]	
3-801	A	21:42	21:42	This sentence doesn't give new information; what uncertainty analysis was done? Replace 'validity' with 'significance'. [Marcia Baker]	
3-802	A	21:45	21:45	Ref. comment #2 above: the section heading "3.3.3 Evaporation" should be "3.3.3 Evapotranspiration" [Michael Hobbins]	
3-804	A	21:45	22:15	In 3.3.3, there was no discussion of evaporation over the oceans, whether it had increased or decreased. Surely, this is where the (net) precipitation over land comes from. I would	

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				be interested to know what are the trends in evaporation over the ocean. Maybe no one is working on that, but even so I think that could be stated [Harry Bryden]	
3-805	A	21:45	22:15	Explicitly mention the phrase "global dimming" in section 3.3.3 and refer to Box 3.1, p43.. [Chris Folland]	
3-806	A	21:45	22:15	Section 3.3.3. This section is perhaps the most acute example of all the chronic problems with this chapter's treatment of evapotranspiration. [Michael Hobbins]	
3-807	A	21:45	22:15	If ETa is driven by solar radiation—as is stated explicitly in this report, (see #19 above)—then how can trends in ETa be unaffected by trends in solar radiation? They can't be unaffected. In fact, trends in ETa result from trends in both the radiative dynamic and the aerodynamic driver (Hobbins et al., 2004). The same is true for trends in Epan: they are also results of trends in radiative dynamic and aerodynamic drivers (Hobbins et al., 2004; Ozdogan and Salvucci, 2004). [Michael Hobbins]	
3-808	A	21:45	22:15	As stated, of course ETa is a function of surface wetness. And ETa in water-limited areas is immediately affected by trends in water availability. The global picture does not stop with water-limited areas, though: in energy-limited areas, ETa is immediately affected by changes in energy availability. I would simply insert the word "also" between "Whether or not ETa decreases or not" and "depends on surface wetness changes." [Michael Hobbins]	
3-809	A	21:45	22:15	To say that Epan does not represent ETa is not strictly true: Epan is a physically observable ETa flux from a saturated surface at a point (which is why I always refer to Epan as a flux, rather than a measure). Certainly, it is not a direct measure of ETa over a region, but it can get you there. This section appears to discard Epan as a hydrologic anomaly just because there is not a linear relationship between ETa and Epan. The correct conception of the relationship between ETa and Epan allows us to tell a lot about regional ETa from Epan (Ref. #3 above). The two fluxes have been shown to be complementary: both increasing with increasing energy availability, but Epan (and the atmospheric evaporative demand it measures) decreasing with the increasing regional moisture availability that increases ETa. Thus, in water-limited environments, ETa increases in response to increasing moisture availability while Epan decreases, while in energy-limited environments, both ETa and Epan decrease with decreasing radiation. [Michael Hobbins]	
3-814	A	21:47	21:47	Ref. comment #2 above: "evaporation" should read "evapotranspiration" [Michael Hobbins]	

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3-816	A	21:47	21:47	there are very limited direct measurements .. I do not even know such measurements at a global scale.. But indirect estimates are generally derived from land surface modelling, calibrated with these measurements at a local scale, which could be used to derive the evolution of actual evaporation on a global scale [Bernard Seguin]	
3-817	A	21:47	22:15	Paraphrasing: "Epan does not represent ETa. Epan is decreasing over US and parts of Europe and Russia due to decreased radiative forcing, not to ETa. ETa trends are a function of surface wetness changes." So, Epan is discounted as a predictor of ETa (perhaps because the Epan trend appears to the authors to be in the wrong direction in relation to trends in ETa) and Epan trends result from radiation trends, while ETa trends result from moisture availability trends. The next few comments outline what is wrong with this picture: [Michael Hobbins]	
3-819	A	21:47		This paragraph is greatly redundant with the box 3.1.If this box is maintained (I am in favour), the paragraph 3.3.3 could be limited to a few sentences. Also, in this part, the hypothesis of an increase of radiation after 1990 needs to be incorporated, as mentioned in 3.4.4.2 about surface radiation [Bernard Seguin]	
3-820	A	21:49	21:49	I suggest you include your Hobbins et al., 2004 reference here (but I would, wouldn't I? It's my paper) [Michael Hobbins]	
3-822	A	21:49	21:52	Decreasing trends in pan evaporation have also recently been reported in Thailand (Tebakari et al. 2005. Journal of Hydrologic Engineering 10: 205-215.) and a forthcoming article also shows a decreasing trend in New Zeland (Roderick & Farquhar 2005, Int J Climatology, in press). The New Zealand article will be published in December 2005 and can be cited by the IPCC. [Michael Roderick]	
3-823	A	21:50	21:50	The downward trend in evaporation over Australia is a line drawn through a W-shaped curve, and so the trend is very dependent upon the end points. Moreover, much of the 'trend' is explained by the reciprocal relationship with rainfall over the period. [Michael Manton]	
3-824	A	21:51	21:51	Add:" accurately" after the word "represent" [Mohamed El-Shahawy]	
3-825	A	21:51	21:51	May be a short recall about the difference between actual and potential evaporation could be welcomed here? [Bernard Seguin]	
3-826	A	21:53	21:55	The decline of solar irradiance has been stopped after the late 1980s—or almost steady in	

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				the last ten years,whereas the increase of pollutants in southern-central China increased much faster in 1990s compared to the previous two decades. This mismatch rejects the idea of attribution of the declined solar irradiance to the increase in local air pollution of China. [Tianjun ZHOU]	
3-827	A	22:2	22:2	Add:" and semiepirical " after the word "empirical" [Mohamed El-Shahawy]	
3-828	A	22:3	22:3	Replace:" using" with " applying" [Mohamed El-Shahawy]	
3-829	A	22:3	22:3	Add:"or applying aerodynamic models using wind speed and humidity measurements (e.g.: El-Shahawy 1987)" at the end of line 3. [Mohamed El-Shahawy]	
3-830	A	22:5	22:5	Ref. comment #2 above: "evaporation" should read "evapotranspiration" [Michael Hobbins]	
3-831	A	22:5	22:7	If, as stated here, the TAR said that an increased atmospheric moisture demand due to higher Tair drove an increase in ETa, it was making the same simplistic mistake as this AR4 draft. Taking the conterminous US as an example: If we measure atmospheric moisture demand by ETp (as stated on line 11), then we have to face the fact that ETp has, in general, decreased. We know this because (i) both the solar radiation and the drying power of the air have, in general decreased (only the Thornthwaite equation--reliant solely on Tair to predict ETp but used throughout this chapter--would indicate that ETp has increased); and (ii) that Epan across the US has decreased (Hobbins et al, 2004). If we measure atmospheric moisture demand by VPD, (as stated in #5 above), heating doesn't imply increased VPD. Over the conterminous US, VPD has decreased in the face of higher Tair (Hobbins et al, 2004), simply because Tdew has risen far faster than Tair. [Michael Hobbins]	
3-832	A	22:5	22:7	Paraphrasing: “ETa increases over most dry regions of USA/Russia during 1950-2000, due to increasing wetness from increased precipitation, and increased ETp from higher temperatures.” Laying aside the fact that the observations of decreasing Epan (at least in the western US: I don’t know about the drier regions of Russia) (Hobbins et al., 2004) conflict with the higher ETp reported here--which the report glosses over because it doesn’t consider that Epan measures anything worthwhile--to say that the increase in Epan is due to higher Tair betrays again the fact that Tdew is considered static and solar radiation is ignored in ETp estimates, as a result of this chapter's implict reliance on Thornthwaite over a more physically defensible approach that encodes solar radiation, VPD, and wind speed, such as the Penman equation. [Michael Hobbins]	

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3-836	A	22:5	22:15	Modelling lake evaporation from climatic variables, Jones et al. (2001) found that the 1960–89 average annual lake evaporation was 1,060 mm pa-1 compared to the long-term average (1863–1989) of 1,032 1,060 mm pa-1. Used as input to a lake water balance model, this record produced a close fit with declining lake level data of three closed lakes in south-eastern Australia, suggesting a recent intensification of a dry climatic episode that began in the mid 19th century. Jones, R.N., T.A. McMahon, and J.M. Bowler (2001) Modelling historical lake levels and recent climate change at three closed lakes, Western Victoria, Australia (c.1840-1990), Journal of Hydrology, 246, 158-179. [Roger Jones]	
3-837	A	22:5	22:15	A substantial investment to "get evaporation measurements right" should be a recommendaion from this Chapter . [Jerry Mahlman]	
3-838	A	22:5	22:16	The large increase in potential evaporation described here as a consequence of warming is in conflict with the decline in measurements of pan evaporation. Also see comment 1 and 21. [Michael Roderick]	
3-839	A	22:5	22:15	This paragraph could, but doesn't, give a straightforward account of changes in understanding since the TAR and of our confidence in the state of the science. [Dian Seidel]	
3-840	A	22:9	22:9	Ref. comment #2 above: "evaporation" should read "evapotranspiration" [Michael Hobbins]	
3-842	A	22:10	22:12	Paraphrasing: “Global land surface evaporation (presumably ETa) decreased from early 70s to mid-90s, when it started to rise again”—that, of course, is what solar radiation did, too. But there’s no connection according to this chapter, which pins the large increase in ETp on warming, saying that we’re therefore going to see drying (the “brownhouse” effect again). Here the observed complementarity of the downward ETa and upward ETp trends aren’t even discussed. Again, the report states that increased ETp is due to increased Tair; no mention of decreased solar radiation during the same period that would run counter to the supposed ETp increase. [Michael Hobbins]	
3-844	A	22:10	22:12	It might be too early to say that increases in atmospheric moisture demand induced by the rapid warming resulted in substantial drying over global land areas. [Guoyu REN]	
3-845	A	22:11	22:11	Why speak here of evapotranspiration, whilst the term evaporation has been used before, which could be understood at the global scale [Bernard Seguin]	

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3-846	A	22:13	22:15	It is not clear how this last reference relates to the framework of this assessment since it seems to be a study about impacts of land cover change on watershed water budgets more than a study of climate impacts either separate or with land cover change. [Richard Fernandes]	
3-848	A	22:17	24:56	Some shortening of section 3.3.4 would clarify the key messages. [Chris Folland]	
3-849	A	22:19	23:16	The description of a large drying trend over NH land measured by PDSI (Dai et al. 2004b) does not appear to be consistent with that of an increasing long-term trend of observed data(Robock et al. 2000). Clarify this point. [Masato Shinoda]	
3-850	A	22:20	22:22	There is no evidence to support the sentence, "Regional inconsistencies in trends are common." and it should be removed. [Alan Robock]	
3-851	A	22:20	22:22	The trend in the Ukraine is not characterized correctly. Rather the sentence should be changed to: "A rare 45-year record of summer soil moisture over agricultural areas of the Ukraine shows a large upward trend, which was stronger during the first half of the period (Robock et al., 2005)." [Alan Robock]	
3-852	A	22:24	22:26	Here we are missing the opportunity to compare the PDSI as well as Keetch Byram Drought Index (KBDI) changes over Eurasia during the past 50-100 years (using papers by Dai et al. 2004b and Groisman et al. 2005b). Both these studies indicate summer dryness increases in Northern Asia. [Pavel Groisman]	
3-853	A	22:25	22:26	The comparison of Iowa with Illinois is incorrect. What is correct is: "While the long Iowa record shows a small downward trend in soil moisture, when compared for the same overlapping period, Iowa and nearby Illinois showed similar upward trends." I suggest just removing both sentences in lines 25-26. [Alan Robock]	
3-854	A	22:28	23:16	The detailed description on the distinct soil moisture trend (Dai et al. 2004a, b) does not appear to be consistent with no statistically significant long-term trend indicated by a similar calculation (Hirabayashi et al. 2005). Clarify this point. [Masato Shinoda]	
3-855	A	22:39	22:39	Suggest to add Qian et al. (2005, already cited in the chapter) to the refs here. [Aiguo Dai]	
3-856	A	22:39	22:39	Replace "Dai et al. 2004b" with "Qian et al. 2005". Add "Ngo-Duc et al. 2005", which is Ngo-Duc, T., J. Polcher and K. Laval, 2005: A 53-year forcing data set for land surface	

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				models. J. Geophys. Res., 110, D06116, doi:10.1029/2004JD005434. [Aiguo Dai]	
3-857	A	22:43	22:54	This strikes me as a major punchline. State it forcefully! [Jerry Mahlman]	
3-858	A	22:43	23:16	A discussion of drought in the context of the well-known and widely used Palmer Drought Severity Index (PDSI) works only in the context of present droughts. Using a PDSI that has been calibrated for the present, one cannot reliably report PDSI values from far in the past or make predictions of far future drought. This is because the PDSI relies on a Thornthwaite parameterization of the ET process, a calibration that reflects climatologically mean conditions of all the dynamics except Tair, which is then allowed to change and for which we have good past records and future predictions. All other dynamics are assumed static. This leads to the "brownhouse" scenario, whereby increasing Tair always results in increasing ETp, therefore a drying environment, and therefore predictions of increased exposure to drought. However, it is easily shown that when the variations in all other dynamics (solar radiation, Tdew, wind speed) are included that such variations often have much stronger signals determining the ETp trends than does Tair, in many cases resulting in a decreasing ETp, and hence a wetting environment. This fact is absolutely fundamental to the analysis of future drought and has enormous implications for policy-makers. This must be stated explicitly. Relying on a PDSI that encodes a Thornthwaite-based ETp is simply not acceptable hydrologic practice at this point. [Michael Hobbins]	
3-860	A	22:44	23:57	Is it possible to explain the inputs to the PDSI? [Marcia Baker]	
3-861	A	22:45	22:45	After first sentence of paragraph add, "Vinnikov and Robock (2002) showed that while there has been a slight upward trend in the PDSI for the US for the past century, there has been no trend in its variability." ref: Vinnikov, Konstantin Y., and Alan Robock, 2002: Trends in moments of climatic indices. Geophys. Res. Lett., 29 (2), doi:10.1029/2001GL014025. [Alan Robock]	
3-862	A	22:45	23:57	Much of the data presented here comes from one or two papers by one researcher. This could be a flag for skeptics---is there no other data? [Marcia Baker]	
3-863	A	22:46		could this refer to a section in ch6 dealing with this? [Gabriele Hegerl]	
3-865	A	22:56	22:56	Delete the reference to CLM-simulated soil moisture as Dai et al. (2004b) only showed PDSI results. Suggest to add the folllowing sentence to the end of this paragraph: "The	

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				PDSI results are consistent with the soil moisture trends from a historical simulation using a comprehensive land surface model (Qian et al. 2005)." [Aiguo Dai]	
3-866	A	22:56	23:10	why to assess the research results from simulation? This chapter is on observation. If you use other methods to calculate you will obtain different results. [Guoyu REN]	
3-867	A	22:56	23:16	The Dai et al. 2004 paper was based on Thornthwaite method of estimating potential evaporation. This is not very useful for climate change purposes. The work in that paper needs to be repeated using either pan evaporation measurements or Penman-Monteith type estimates along the line of Chen et al. (2005, Climate Research, 28, 123-132). Also see comment 21. [Michael Roderick]	
3-868	A	23:0		These regional, large-scale "trends" are bedeviling the region-scale detection-attribution problem. Clearly, a major IPCC goal must be to attack the major barrier of over-interpreting regional trends that are not really relevant to our understanding of the "true regional climate trends". [Jerry Mahlman]	
3-869	A	23:1	:17	A figure for spatial changes indrought would be nice here [Gabriele Hegerl]	
3-870	A	23:2	23:4	The decadal/multi-decadal variability of the precipitation regime should be noted [Aristita Busuioc]	
3-871	A	23:9	23:11	On this subject, the paper Bordi et al. 2004 (Theor. Appl. Climatol., 77, 125-138) analyzes the SPI (standarized precipitation index) variability for station data in the Central Europe (Germany) and in the Mediterranean (Sicily). [Paolo Michele Ruti]	
3-873	A	23:16	23:16	Please, insert in the end of the paragraph results for Northern Eurasia using findings about increasing potential forest fire danger indices (Nesterov, Zhdanko, and KBDI using Groisman et al. 2005b), PDSI (Dai et al. 2004b) and drought indices of Mescherskaya and Blazhevich (1997 updated up to 2004). [Pavel Groisman]	
3-874	A	23:18	23:19	This sentence is not clear. [Marcia Baker]	
3-875	A	23:18	23:25	The work by Zou et al. (2005) did not show any significant role played by change in temperature. Rather precipitation change is dominantly important for the dying trend of North China (Zou et al., 2005). For this region of China, significant decrease in observed pan evaporation has been related to decline in sunshine duration and wind speed. It is	

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				more uncertain for impact of air temperature and relative humidity, but they are generally considered to have played a minor role in causing the decrease of pan evaporation and theoretically calculated evaporation over the past 40-50 years in this region (Guo and Ren, 2005, Recent change of pan evaporation and the possible climate factors over the Huang-Huai-Hai Watersheds, China, Advances in Water Science, 16 (5), 666-672 (in Chinese); Chen, D. Gao, G., Xu, C., Guo, J. and Ren, G., 2005, Comparison of the Thornthwaite method and pan data with the standard Penman-Monteith estimates of reference evapotranspiration in China, Climate Research, 28, 123-132). [Guoyu REN]	
3-876	A	23:27	23:28	The sentence about the southwest Asia drought isnt very interesting. Suggest deletion. [Neville Nicholls]	
3-877	A	23:27		The results by Hoerling and Kumar (2003), regarding the drought 1998-2002, which extended to parts of North America, southern Europe and southwest Asia could be mentioned here. [Annarita Mariotti]	
3-879	A	23:35	23:35	The recent droughts in Europe (associated to the heat wave of 2003, and significant in 2005, especially in France, Spain and Portugal) could also be mentioned here [Bernard Seguin]	
3-880	A	23:35		A paragraph about the frequency of drought episodes in Europe, including the mediterranean region in particular as well as a mention of summer 2003 (concerning the drought and not the heat) should be added here. [Martine Rebetez]	
3-881	A	23:35		Add a paragraph concerning Europe, including: "On the Southern side of the swiss Alps, drought episodes have increased in frequency and persistence during the 20th century and have been particularly frequent and long during the 1940s as well as since 1981 (Rebetez, 1999). Reinhard et al. (2005) show a seasonal increasing trend in all climatic variables favourable to drought, including the length of episodes without precipitation, sunshine duration and temperature, and a decrease in relative humidity. [Rebetez, M, 1999: Twentieth century trends in droughts in southern Switzerland. Geophys. Res. Letts. 26: 755-758] [Reinhard M, Rebetez M, Schlaepfer R, 2005: Recent climate change: Rethinking drought in the context of Forest Fire Research in Ticino, South of Switzerland. Theor. Appl. Clim., 82 (1-2): 17-25] [Martine Rebetez]	
3-882	A	23:35		Add a paragraph concerning Europe, including: " The drought was extreme in summer 2003 in western and central Europe with the lack of precipitation being most severe in summer but lasting for a period of 17 months (Rebetez et al. 2005)." [Rebetez M, Mayer H, Dupont O, Schindler D, Gartner K, Kroppe J, Menzel A, 2005: Heat and drought 2003	

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				in Europe : a climate synthesis. Ann. For. Sc., accepted] [Martine Rebetez]	
3-883	A	23:40		Delete "recent", since you give the years of the drought. [Neville Nicholls]	
3-884	A	23:43	24:7	Somewhere in this discussion of streamflow data, Gedney et al. (2005, Nature, submitted) should be cited. [Nathan Gillett]	
3-885	A	23:57	24:3	The statement is conflict with the previous conclusion that precipitation over that region is increasing! [Guoyu REN]	
3-886	A	24:0		Attribution of the causes of secular regional climate trends remains a major challenge to our understanding about true regional climate trends due to increasing radiative forcing of the planet. [Jerry Mahlman]	
3-887	A	24:11		'e. g.' to 'e.g.' [Ian Simmonds]	
3-888	A	24:33	24:40	This sort of water balance approach to ET estimation is currently the most reliable method for large areas. It may be useful to highlight it also in the evaporation section and perhaps move part of this paragraph there. [Richard Fernandes]	
3-890	A	24:37	24:37	You might find useful to introduce here, some comments related to the relationship between Danube river discharge in its lower basin and large scale circulation variability, mostly by the NAO reported by Rimbu N., C. Boroneant, C. Buta, M. Dima (2002): Decadal variability of the Danube river streamflow in the lower basin and its relation with the North Atlantic Oscillation. International Journal of Climatology, 22, 1169-1179.: "the decadal variations of Danube river flow in its lower basin are in good agreement with the decadal variations of precipitation in its catchment, being largely controlled by North Atlantic processes, particularly by the NAO. [CONSTANTA-EMILIA BORONEANT]	
3-891	A	24:37	24:40	Our works show that not only temperature increased but precipitation also decreased in the Yellow River basin, and the decrease in precipitation has significantly contributed to the decrease of runoff. The decrease of precipitation is very significant in the middle and lower reaches of the basin (Ren, G. Y., Guo, J., Xu, M. Z., et al., 2005, Climate changes of Mainland China over the past half century, Acta Meteorologica Sinica, 63 (5) (in press in Chinese); Zhai, et al., 2004; Ren, G., Wu, H. Chen, Z., 2000: Spatial pattern of precipitation change trend of the last 46 years over China, Quarterly J. Appl. Met., 11, 322-330)	

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				[Guoyu REN]	
3-892	A	24:38	24:40	Here we see the faulty brownhouse scenario--that increasing Tair suggests drying--again. [Michael Hobbins]	
3-894	A	24:49	24:49	land, areas" --> "land areas [Richard Allan]	
3-895	A	24:49	24:56	Here we see the faulty brownhouse scenario--that increasing Tair suggests drying--again. [Michael Hobbins]	
3-897	A	24:51	24:52	"seasonal decreases in land precipitation since the 1950s...": this seems to be in contradiction with Figures 3.3.1, 3.3.2 and 3.3.3. Precip appears to be stable since 1950, increasing since 1900 in these figures. [Reinhard Böhm]	
3-898	A	24:56		Is it getting wetter anywhere based on the PDSI? (worry about selective reporting..) - also, a reference to the section on pcp extremes here may be nice [Gabriele Hegerl]	
3-899	A	25:0		Section 3.3.5. This is a poorly developed section. Relation between temp. and precip also depends on circulation changes. I suggest to delete the section here and integrate it with section 3.9 [Fons Baede]	
3-900	A	25:0		Section 3.3.5: the consistency between temperature and precipitation depends on the Clausius Clapeyron relationship, but also on the dynamical regime (convective in the Tropics, baroclinic in the mid-latitudes). In my opinion, this section should divide more clearly the consistency in the Tropics and in the mid-latitudes. [Paolo Michele Ruti]	
3-901	A	25:1	25:25	This section is not really reporting observations. Perhaps it could go somewhere else, for instance in the executive summary? [Marcia Baker]	
3-902	A	25:1		Horizontal temperature gradients have also been found to be an important factor in explaining precipitation amounts – see the recent paper: Sapiano, M.R.P., D.B. Stephenson, H. J. Grubb, and P. A. Arkin, 2005: Diagnosis of Variability and Trends in a Global Precipitation Dataset Using a Physically Motivated Statistical Model, J. Climate, accepted subject to revisions. [David Stephenson]	
3-903	A	25:3	25:18	This is a nice analysis. [Jerry Mahlman]	
3-904	A	25:13	25:18	Note the following recent study on the summer temperature-precipitation relationship for	

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				the Sahel; Shinoda, M. and Y. Yamaguchi: Influence of soil moisture anomaly on temperature in the Sahel: A comparison between wet and dry decades. Journal of Hydrometeorology, 4(2), 437-447, 2003. They examined the mechanism of soil moisture causing the relationship with a time lag. [Masato Shinoda]	
3-905	A	25:20	25:25	such regional examples are dangerous! Just compare the JJA-temperature map of Figure 3.2.10 with the JJA precip map in Figure 3.3.3 for the Sahel-region: both increase for 1979-2004. [Reinhard Böhm]	
3-906	A	25:20	25:25	In Australia it is true that this relationship holds generally, but the warming trend in Australian temperatures since about 1950 has not been accompanied by lower precipitation. So, the relationship, although strong on interanual scales, has not worked in recent decades on trends. [Neville Nicholls]	
3-907	A	25:22		... increased precipitation intensity ... [Jerry Mahlman]	
3-908	A	25:22		'a more positive NAO' will not be understood by most readers. [David & David Wratt & Fahey]	
3-909	A	25:23	25:25	Here we see the faulty brownhouse scenario--that increasing Tair suggests drying--again. [Michael Hobbins]	
3-911	A	25:23	25:25	During the Australian drought, there is llittle rain and hence little evaporation. Consequently, Hence, evaporation cannot be enhanced as stated in the text. Instead, potential evaporation is enhanced. The empirical equivalent of potential evaporation is pan evaporation which was also enhanced during the drought. Hence, just change evaporation to potential evaporation and that will fix the problem. [Michael Roderick]	
3-912	A	25:29		It may be wise to further highlight that the effect of aerosols on precipitation is addressed in Chapter 7. I was originally expecting to find this here. [Steven Siems]	
3-913	A	25:29		Question 3.2: The answer addresses more how precipitation 'does or might change' rather than how precipitation 'is' changing. Relevant to this point is that Q3.3 in opening line 24 states that 'precipitation over land has decreased' but no straightforward similar conclusion is presented in Q3.2. [David & David Wratt & Fahey]	
3-914	A	25:31	25:37	As El Nino is mentioned, PDO/IPO influences on precipitation on decadal timescales is worthy of a mention - especially as large changes are seen in precipitation in many areas	

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				of the Pacific Basin in response. [M James Salinger]	
3-915	A	25:31	25:37	(Format): This paragraph should be in italics since it is the "headline answer" to the question. [David & David Wratt & Fahey]	
3-916	A	25:32		The concept of El Nino will not mean much to many desired readers in this context. Suggest replacing with 'changes in winds and sea surface temperatures (e.g., El Nino years)'. [David & David Wratt & Fahey]	
3-917	A	25:33	25:33	"in some places" is too generic, it doesn't give any idea if it is statistically significant or not. [Franco Desiato]	
3-918	A	25:33	25:34	Question 3.2: How is precipitation changing? The answer to this question includes statements of things that have not been discussed as of yet in this Chapter. For instance, "Pronounced long-term trends have been observed in some places, with widespread increases in heavy rains, even in places where total amounts have decreased." Yet, there has been no discussion about rainfall intensity to this point or how it related to annual precipitation changes. Thus, this is stated without references. [Jeffrey Kueter]	
3-919	A	25:33	25:33	The NAO could also be mentioned here. [Annarita Mariotti]	
3-920	A	25:34	25:37	The first of these two sentences refers to increases in some regions of both dry conditions / droughts and wet conditions / floods. The second sentence says these changes are associated with increased water vapour in the atmosphere. But is it correct to attribute more droughts to an increase of water vapour in the atmosphere? We suggest reversing the order of these two sentences, so that the increased water vapour in the atmosphere sentence follows (and refers to) the earlier sentence about widespread increase in heavy rains. [David & David Wratt & Fahey]	
3-921	A	25:35	25:35	I suggest to use the term 'occurrence' rather than 'risks' as far as the sentence refers to past rather than future [Bernard Seguin]	
3-922	A	25:36	25:37	Insert 'in part' after 'associated' - increased atmospheric water vapour doesn't explain increased drought. [Nathan Gillett]	

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3-923	A	25:36	25:37	Increased water supply in the atmosphere due to global warming suggests the opposite dynamic to the faulty brownhouse scenario. [Michael Hobbins]	
3-925	A	25:36	25:37	.. arising from the recent warming of the world's oceans, especially in lower latitudes. [Jerry Mahlman]	
3-926	A	25:36	25:37	I am not a raving climate skeptic but I think statements such as this are unwarranted and damaging to the report's credibility. Frankly, the jumble of precipitation data shown in the previous sections offers no clear picture and I see no possibility of attributing it to global warming. Not all of the changes are even consistent with model predictions (a global-warming explanation of Sahel drought would not predict, for example, the recent drought abatement even as global temperatures have really picked up). [Steven Sherwood]	
3-927	A	25:39	25:46	This strikes me as an unnecessary tutorial. [Jerry Mahlman]	
3-928	A	25:40		The phrase 'Most of the time precipatation is not formed' lacks context, so suggest changing to 'is not occurring at a given location'. [David & David Wratt & Fahey]	
3-929	A	25:48	25:57	This is much better. It now sets up the point stated so awkwardly back on pages 16, lines 52-55. Redo both parts! [Jerry Mahlman]	
3-930	A	25:48	26:12	Another direct influence on precipitation of GHG increases is the enhanced IR optical depth of the troposphere. This reduces IR cooling to space and tends to reduce precipitation (Allen and Ingram, 2002). Indeed if GHGs are increased in a model, but SSTs are held fixed, the precipitation decreases (Yang et al., 2003, J. Clim., 16, 2419-2423). [Nathan Gillett]	
3-931	A	25:49	25:50	Here we see the faulty brownhouse scenario--that increasing Tair suggests drying--again. [Michael Hobbins]	
3-933	A	25:49	25:57	An omitted factor giving rise to the difference between land and ocean surface temperature changes is the contribution of latent heat of evaporation. While the heating effects of climate change will be uniformly distributed, the latent heat of evaporation will depress the temperature rise over ocean surface in comparison to that over land surface. The thermodynamics of radiative forcing remain consistent. The resultant increase in energy over land surface is reflected in higher temperatures, but the energy increase over ocean surface is reflected only partially by temperature rise, and partly by latent heat	

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				stored by the increased concentration of (non-condensed) atmospheric water vapour. [David Wasdell]	
3-934	A	25:50	25:50	Replace:"oceans" with "large water surfaces of moist soils" [Mohamed El-Shahawy]	
3-935	A	25:52	25:52	Add: "often" after the word"summers" [Mohamed El-Shahawy]	
3-936	A	25:52	25:54	Where is the observational evidence for increasing droughts? Also see comment 21. [Michael Roderick]	
3-937	A	25:53	25:54	"Warming accelerates land-drying worldwide and increases the the potential incidence and severity of droughts, which is observed to be happening worldwide." This is a modeling result, not an observation. It is "observed" only in that a Thornthwaite-encoded PDSI meters it using past observations of precipitation and Tair. [Michael Hobbins]	
3-939	A	25:53	25:54	The sentence may need to be revised. It does not seem to be happening worldwide… [Guoyu REN]	
3-940	A	25:53	25:53	And vice versa: Land-surface drying accelerates warming so that a positive feedback is in action (see also Box 3.5.5 "Heat wave in Europe, summer 2003). [Christian-D. Schoenwiese]	
3-941	A	25:54		Suggest the following re-write: "The Claussius - Clayeyron equation, a well-known relationship derived from the First Law of Thermodynamics, determines that the……" [Anthony Lupo]	
3-942	A	25:55	25:55	Add:" in normal ranges of temperature and pressure, " after the word "that" [Mohamed El-Shahawy]	
3-943	A	25:56	25:56	The sentence "Observations suggest that relative humidity remains about the same overall,…" is not acceptable. Where is the respective global dataset of relative humidity? Is the statement based on homogenised data (in the respective boxes there is only temperature and precipitation mentioned)? Does the statement refer to global or regional scale? Her is a regional example: Auer et al., 2005b show (Figure 21) that in Central Europe the increase of vapour pressure does not balance the warming trend there and relative humidity decreases significantly for 1901-2000 as well as for 1975-2000. [Reinhard Böhm]	
3-944	A	25:56	26:1	RH may be constant in the global troposphere overall, but it cannot be assumed to be constant at any particular location at the surface. It varies, but little, due to its insensitivity. It does not imply that there is no trend in actual vapor pressure. What does it mean to say that increased water vapor in the atmosphere results from increased drying at the surface? You are implying a shift in water storage from the surface to the atmosphere,	

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				leaving a more moist atmosphere and a drier surface. This storage change could only evolve and be sustained if the flux to the atmosphere (i.e., ETa) increased, while that from the atmosphere (i.e., precipitation) decreased, a combination that has not been observed. [Michael Hobbins]	
3-946	A	25:56	26:1	The statement on the increased surface drying does not agree with the poor reliability of the soil mositure analysis stated in section 3.3.4 (lines 28-41). [Paolo Michele Ruti]	
3-947	A	25:57	25:57	Add: "when water vapour sources are available" after the word " atmosphere" [Mohamed El-Shahawy]	
3-948	A	26:0		In section 3.4- Changes in the Free Atmosphere- the deficiencies of MSU are discussed. The relatively new sounding technique, GPS radio occultation (GPS RO), can make a significant contribution to resolving the problems associated with infrared and microwave sounders, yet it is not mentioned until only very briefly at end of the chapter (p. 3-119). The potential of GPS RO should be mentioned in section 3.4. Given the long-standing controversy and the policy implications of this controversy, resolving this issue, which RO can certainly do for the upper troposphere and lower stratosphere at least, should be mentioned. RO can also determine the tropopause height and structure on a global basis as no other sounding technique can. GPS RO could also be mentioned in section 3.5.A.2 p. 3-117. Additional references GPS Ro should be included, for example: [Richard Anthes]	
3-949	A	26:0		Yuan, L., R. A. Anthes, R. H. Ware, C. Rocken, W. D. Bonner, M. Bevis, S. Businger, 1993: Sensing climate change using the Global Positioning System. Journal of Geophysical Research-Atmospheres, 98, No. D8, 20 August 1993, 14,925-14,937. Businger, S., Chiswell, S., Bevis, M., Duan, J., Anthes, R., Rocken, C. Ware, R., Exner, M., VanHove, T. and F. Solheim,1996: The promise of GPS in atmospheric monitoring. Bulletin of the American Meteorological Society, 77, 1, 5-18. Ware, R., M. Exner, D. Feng, M. Gorbunov, K. Hardy, B. Herman, Y. Kuo, T. Meehan, W. Melbourne, C. Rocken, W. Schreiner, S. Sokolovskiy, F. Solheim, X. Zou, R. Anthes, S. Businger, and K. Trenberth, 1996: GPS Sounding of the Atmosphere from Low Earth Orbit: Preliminary Results. Bulletin of the American Meteorological Society, 77, No. 1, January 1996, 19-40. Rocken, C., R. Anthes, M. Exner, D. Hunt, S. Sokolovskiy, R. Ware, M. Gorbunov, W. Schreiner, D. Feng, B. Herman, Y.-H. Kuo and X. Zou, 1997: Analysis and validation of GPS/MET data in the neutral atmosphere. J. Geophys. Res., Vol. 102, D25, 29,849-29866. [Richard Anthes]	

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3-950	A	26:0		An excellent reference/review book that summarizes the history of GPS RO and the applications to climate, weather and space weather may be found in: Lee, L.-C., C.R. Rocken and R. Kursinski, 2000: Application of Constellation Observing System for Meteorology, Ionosphere and Climate. Springer, New York, 384 pp. [Richard Anthes]	
3-951	A	26:0		Section 3.4.1. Much of the detail here could be moved to the Appendix section 3.A.5; some is duplicative of text in that section and could be deleted here. [Melissa Free]	
3-952	A	26:1	26:12	A nice, well justified, confident statement. [Jerry Mahlman]	
3-953	A	26:1	26:2	This line states that water vapor increased by about 5% over the oceans in the atmosphere. However, according to Chapter II (section 2.3.8) the radiative forcing associated with water vapor increase is small. Do these results fully match? Some further explanation (either here or in chapter II) on the increase of water vapor versus the corresponding radiative forcing would be helpful. See also section 3.4.2 in Chapter III for the consistency check. [Philippe Tulkens]	
3-954	A	26:2	26:2	Add: "in the atmospheric low lying layers" [Mohamed El-Shahawy]	
3-955	A	26:2	26:2	Delete: "in the atmosphere" [Mohamed El-Shahawy]	
3-956	A	26:4	26:9	This chapter should not talk about climate model simulations. and it may be early to say that warming has led to more intense precipitation events. [Guoyu REN]	
3-957	A	26:7	26:7	"Ironically" is not the right word. Please replace it with a more suitable one. [FILIPPO GIORGI]	
3-958	A	26:7	26:7	Ironically : unusefull term [Jean-Marc Moisselin]	
3-959	A	26:7	26:9	This passage suggest that the European events of 2002 and 2003 have been attributed to the warming trend. I don't think this is an accurate implication. [Neville Nicholls]	
3-960	A	26:8	26:8	2003 and not 2002 [Bernard Seguin]	
3-961	A	26:9		cite "(see box 3.5.4 and 3.5.5)"	

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				[Jürgen Grieser]	
3-962	A	26:10	26:10	Replace: "El Nino" with "ENSO" [Mohamed El-Shahawy]	
3-963	A	26:14	26:14	Excellent figure as it shows spatially where precipitation is changing. [M James Salinger]	
3-964	A	26:16	26:23	This is well done, but it can profit from a more clear statement on how it relates to the regional "trend detection problem".26 [Jerry Mahlman]	
3-965	A	26:16	26:18	Aerosols might decrease the potential (or pan) evaporation rate but not necessarily the evaporation rate over land. [Michael Roderick]	
3-967	A	26:21	26:21	You might useful to consider here the reference "Rimbu N., H. Le Treut, S. Janicot, C. Boroneant and C. Laurent (2001): Decadal precipitation variability over Europe and its relation with surface atmospheric circulation and sea surface temperature, Quart. J. Royal Met. Soc., vol. 127, no. 572, part B, 315-329". The main finding of this paper is that the most important part of the decadal winter precipitation variability (about 48%) over Europe is described by two modes: a NAO like pattern explaining about 32% of the variance and a “decadal ENSO” like pattern explaining about 16% of the variance. [CONSTANTA-EMILIA BORONEANT]	
3-968	A	26:24	26:25	Change "The prolonged drought in the Sahel from the late 1960s to the late 1980s continues", which makes no sense, to "The prolonged drought in the Sahel, which was pronounced from the late 1960s to the late 1980s, continues" or to something else that makes sense. [Adrian Simmons]	
3-969	A	26:27		correct: "estmates" to "estimates" [Hartmut Grassl]	
3-970	A	26:27		spelling - "estimates" [Neville Nicholls]	
3-971	A	26:30	26:39	A nice analysis and interpretation, but it doesn't speak directly to the bedeviling problem of attribution of such phenomena to detection /attribution of secular region-scale "climate trends". [Jerry Mahlman]	
3-972	A	26:30	26:33	Is this correct or is the decline in snow pack due to increased melt (this is what seems to be happening in Australian snow fields - no strong reduction in snow fall, but increased melt. [Neville Nicholls]	

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3-973	A	26:35	26:38	what do you want to say? Does it have anything to do with global warming in case of paleo-records indicating that the events also occurred in the far past? [Guoyu REN]	
3-974	A	26:44	26:57	Delete this whole paragraph.. It is grossly exaggerated. Uncertainties in atmospheric measurements are often less than the uncertainties in surface measurements. They can claim global coverage, which the surface measurements cannot. [Vincent Gray]	
3-975	A	26:44		Section 3.4.1. This is an important section. Why not a box explaining what precisely is new since TAR? Problems with upper-air temps have been used often by greenhouse sceptics. [Fons Baede]	
3-976	A	26:46	26:57	The paragraph doesn't clearly explain the relation to the above title [Mohamed El-Shahawy]	
3-977	A	26:46	26:57	This paragraph is too long - don't need this level of detailing of caveats. [Neville Nicholls]	
3-978	A	26:46	26:46	Outside of a very few people who claim to know their dataset is "the way and the light and the truth" there are widely recognised uncertainties in both radiosonde and satellite records. Differentiating by the use of the word particularly is therefore unfair and this word should be dropped. [Peter Thorne]	
3-979	A	26:48		Suggest changing "obfuscate" to "obscure". [Melissa Free]	
3-980	A	26:51	26:51	seemingly reasonable" --> "subjective [Richard Allan]	
3-981	A	26:51	26:54	I agree that it is difficult to make defensible judgements on which of multiple data sets are closest to the true climate evolution - but this has been done anyway and is not justified appropriately (see my points above) [Mark McCarthy]	
3-982	A	26:55		Suggest changing "observe" to "constrain". [Melissa Free]	
3-983	A	26:56		After "ground-truth" add "and for improved reanalyses to blend optimally the varied data provided by the changing in-situ and satellite-based observing systems" [Adrian Simmons]	
3-984	A	27:0		What are the relative accuracies of radiosonde soundsings at different levels. Are upper troposphere measurements of adequate accuracy? [Richard Allan]	

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3-985	A	27:2	27:57	Radiosonde data is valuable, though it also has some problems like the surface observation data. Wang et al. (2005) shows a small increase in annual temperature in the middle and lower layers of troposphere over China since 1960. The mid- to lower troposphere (850-400hPa) witnesses only a slight warming in terms of annual mean temperature, with a trend of only 0.05?/10a for the period of 1961-2004, while upper troposphere (300-150hPa) and lower stratosphere (100-50hPa) have experienced a significant decrease in temperature at -0.17?/10a and -0.22?/10a. Increase in annual mean surface air temperature is an order of magnitude higher than that in the middle and lower layers of troposphere. We do not agree to the statement that radiosonde data contain diurnal cycle influences that lead to spurious cooling throughout the atmosphere from 1979 to 1997, because the mid- to lower troposphere is experiencing more significant warming during this period in mainland China, with a trend of only 0.25?/10a. (Wang, Y. and Ren, G. Y., 2005, Change in free atmospheric temperature over China during 1961-2004, Climate and Environmental Research, 10 (4) (in press in Chinese)). [Guoyu REN]	
3-986	A	27:2	28:2	This whole section is devoted to an unjustified attempt to try to destroy confidence in radiosonde measurements. It should be rewritten with less obvious prejudice. Radiosondes are every bit as reliable as surface measurements. [Vincent Gray]	
3-987	A	27:2		Add “And are summarised in the following Table”, and supply it! [Vincent Gray]	
3-988	A	27:3	28:1	A page on the problems with radisondes and how to solve them is much too much detail - replace it by one short paragraph. [Neville Nicholls]	
3-989	A	27:3		Start off by telling us what the measurements have shown.There are important differences from the surface record. [Vincent Gray]	
3-990	A	27:5		Change "five" to "six" - see comment 17. [Adrian Simmons]	
3-991	A	27:8	27:9	Important to make the point that this is an application to the techniques that were applied to datasets considered in or available at the time of the TAR. This paragraph with a bit of tweaking should give a justification for why further work was deemed to be required subsequent to the TAR to improve our understanding. At the moment it feels like it falls short of this. I urge a redraft to be explicit that this is the raison d' etre for subsequent efforts and tell this story more coherently. [Peter Thorne]	
3-992	A	27:14	27:16	This sentence is out of context with the rest of the paragraph. And as the dataset relies	

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				upon LKS data it makes sense to discuss LKS first from a narrative point of view. [Peter Thorne]	
3-993	A	27:18	24:31	The paragraph seems to go into too much detail of the LKS dataset. The general approach with references and its consistency or otherwise with other datasets would be more succinct. [Richard Allan]	
3-994	A	27:18	27:59	I have huge respect for the heroic efforts of the data diagnosticians to get all of the garbage out of the data sets. Quite frankly, thes people deserve far better data, be they sonde or satellite, than they have been given. We don't want to still be dealing with crappy data in, say 2020. [Jerry Mahlman]	
3-995	A	27:19	27:20	Clarify what "value has been verified using independent satellite temperatures". We now know that the UAH T2LT trends before the most recent 2005 version are totally wrong, especially in the tropical region. [Qiang Fu]	
3-996	A	27:21		Add "estimates of" both after "enhanced" and after "reduced" [Adrian Simmons]	
3-997	A	27:23	27:24	I was confused here as to which layer(s) LKS data were consistently lower in trend. They match UAH in the T2LT, but are consistently lower than both UAH and RSS in the T2 and T4 layers. [John R Christy]	
3-998	A	27:24	27:25	It could be misleading to compare the radiasonde with "one satellite record" that was wrong. Also the statement in line 25 is inconsistent with that in line 20. [Qiang Fu]	
3-999	A	27:26	27:29	There is a minor error in the description of the relation between LKS data (ending in 1997) and RATPAC (up to date). The source of the data for updating was the IGRA (Integrated Global Radiosonde Archive, Durre et al. 2005). The result is RATPAC, which is is not so much an archive as a set of data products. [Dian Seidel]	
3-1000	A	27:27	27:29	The data are taken from the IGRA archive, not RATPAC. RATPAC is the resulting dataset. [Melissa Free]	
3-1001	A	27:29	27:31	The biases are not neccesarily the result solely of instrument changes, and the evidence is not conclusive, so it would be better to say: "cooling biases from changes in instruments and procedures may remain" [Melissa Free]	

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3-1002	A	27:36	27:39	The material about inadequacy of linear trends would go better somewhere else. [Melissa Free]	
3-1003	A	27:41	27:51	I have a very different opinion of this section (i.e. concerning Sherwood et al.) having had considerable experience with these same sondes. This is not a "major new development." Sherwood et al. did not consider the spurious nighttime warming jumps due, for example, in Australia and SE Asia sondes, thus giving a result that was spuriously warm in trend. Christy and Norris 2004, applied corrections to both day and night (as did LKS and Thorne et al. 2005), and in agreement with Sherwood et al., found most tropospheric sonde corrections dealt with spurious cold jumps, but there were significant warm jumps too, especially in the night sondes. In particular, the Mark II to Mark III introduces a small spurious warm jump (1982-1983) while the Mark III to Vaisala RS-80 introduces a large tropospheric jump (up to 1 K at some stations during 1987-1989). Thus, the Sherwood method is contaminated by this situation, especially in the 1979-1997 period. I will be forwarding the IPCC authors a paper which will explain in much greater detail the issues I'm bringing up here. [John R Christy]	
3-1004	A	27:44	27:51	Note that if night values are also affected by inhomogeneities, these bias estimates will not be reliable. [Melissa Free]	
3-1005	A	27:44	27:51	As written this is entirely misleading. Sherwood et al. found problems in the raw data and not in the datasets being used in the report. Whether the problems persist in the treated data, and if so to what extent, is conjecture at this point (and I don't think the IPCC report should be dealing in conjecture). Given this it is wrong to imply, as the current text does, that residual biases remain in the adjusted datasets. The text needs to be modified to make this point clearly. The work by Randel and Wu could also be quoted. This did consider the RATPAC product and did find residual problems in the tropics, but did not find that they were limited to night-time alone. This latter point is also a very important one to make. Day-night will not deliver us on its own to some scientific panacea where all is well with the world - there are multiple problems with the data and it is important to make this point clearly and strongly within this text. So, the paragraph needs to be expanded and clarified to give better, more balanced, discussion. Radiosonde analyses are almost certainly not perfect, but concentrating solely on the day-night aspect will not yield a homogeneous dataset. [Peter Thorne]	
3-1006	A	27:45		"almost certainly" is too much. Try "probably primarily". [Melissa Free]	
3-1007	A	27:45		correct: "has" to "have"	

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				[Hartmut Grassl]	
3-1008	A	27:46	27:46	Typing error: “have” is repeated two times [Michele BRUNETTI]	
3-1009	A	27:46	27:47	The effects are due to more than "improved sensors"-- also new or improved radiation corrections. [Melissa Free]	
3-1010	A	27:46	27:46	One "have" should be eliminated [Eugene Rozanov]	
3-1011	A	27:46		delete: "have" [Hartmut Grassl]	
3-1012	A	27:49	27:51	The numbers quoted refer only to uncorrected radiosonde records - The homogeneity corrections applied in both LKS and HadAT act to increase the global warming trend for the period from 1979. Randel and Wu note that there are still problems at some of the LKS stations but it is likely that the homogenisation has removed at least some of the day-time biases. Sherwood (2005) does not provide a quantification of the expected impact on these homogenised datasets. [Mark McCarthy]	
3-1013	A	27:57		1958 appears again - see comment 17. [Adrian Simmons]	
3-1014	A	28:0	30:10	Simple punchline: Call for IPCC to advocate for a global campaign to fix this embarassment and make it an upfront punchline of Chapter 3. [Jerry Mahlman]	
3-1015	A	28:21		Insert after “radiosondes” “and surface measurements” [Vincent Gray]	
3-1016	A	28:26		Add "at a particular location" after "made". As it stands the sentence makes no sense, as satellite measurements are made throughout the day. [Adrian Simmons]	
3-1017	A	28:27	28:29	After the sentence in (3), consider to add "Since the calibration target temperatures vary with the satellite diurnal drift, the satellite calibration and diurnal drift corrections are intricately coupled together (Fu and Johanson 2005)". [Qiang Fu]	
3-1018	A	28:32		There needs to be a Table of temperature results for MSU and radiosondes, to compare with Table 3.3 for surface results [Vincent Gray]	
3-1019	A	28:35	28:35	Consider to change "in the satellite measurement of" to "in the analyses of satellite measurement for".	

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				[Qiang Fu]	
3-1020	A	28:35	28:42	Agree that the most significant progresses in the analyses of satellite measurements for temperatures since TAR are (1) a number of newly constructed datasets and (2) new insights of the stratospheric influence on the tropospheric records. The latter also indicates the problem of UAH T2LT (Fu et al. 2004; Fu and Johanson 2005) which was further confirmed in the study by Mears and Wentz (2005). [Qiang Fu]	
3-1021	A	28:38	28:38	Fu and colleagues have not specified the influence of the stratosphere, nor to my knowledge have they ever claimed to! They have simply undertaken a linear re-combination of two channels to minimise the stratospheric influence (minimise and not remove, specify, or characterise!). Suggest that this passage is deleted and then text placed where TLT is discussed outlining the two approaches (statistical vs. limb differencing) and their complimentary nature. The current text gives undue prominence to the statistical approach which (by implication) has the least physical justification. This is not particularly sensible. A more balanced discussion in the context of "ways to remove stratospheric influences from channel 2" (I'm sure a better title is possible) would cause less consternation amongst the community at large. It is probably best to delete what is there at present for both 2LT and the statistical retrieval and get Carl Mears, John Christy and Qiang Fu to iterate some text if this can be arranged under the title given above or something similar. I think this would improve the analysis immensely. [Peter Thorne]	
3-1022	A	28:40	28:40	Add "analyses" after "datasets". [Qiang Fu]	
3-1023	A	28:44	28:50	There's got to be a more intelligible way of writing / summarising the different efforts to date to create MSU data sets. Maybe including it as a table detailing the MSU groups and which channels they have constructed datasets for would be better. So channels (4, 2, 3, 2LT) as columns and dataset producing groups (RSS, UAH etc.) as rows. V&G do not produce a channel 4 retrieval. RSS create a channel 3 retrieval. UAH and RSS produce 2LT. These "differences" could be easily and more rationally incorporated in a table than within the text. Within the table the trends may be given along with their published construction uncertainty estimates rather than tick / cross to double the information content / utility. The report should also acknowledge the efforts by Prabhakara(sp?) et al. to create a channel 2 retrieval even though this is not updated in real time. [Peter Thorne]	
3-1024	A	28:48	28:48	Remove "and surface". [Qiang Fu]	
3-1025	A	28:48	28:49	Confusing appearance of the word "surface " twice in these lines.	

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				[Adrian Simmons]	
3-1026	A	28:52		Provide a Table of MSU results, for the lower troposphere, for comparison with the surface record for the same period. [Vincent Gray]	
3-1027	A	28:55		Delete “large” [Vincent Gray]	
3-1028	A	28:56		Reference to orbital drift and diurnal cycle is repetitive; they are covered earlier on page in lines 25-27. [Adrian Simmons]	
3-1029	A	29:1	29:57	The issue here is important but the discussion is too technical and detailed; it should be summarized. [Marcia Baker]	
3-1030	A	29:4	29:5	Need to update UAH work in the light of reports in Science, etc August 2005. [M James Salinger]	
3-1031	A	29:8	29:10	This is a very dangerous statement to make and should be deleted. It is effectively conjecture because, depending upon the timeseries properties it may be preferable to discount short overlaps. The available information is, in my opinion, insufficient to take a position on the issue. It is fine to state that it is an issue, and okay to suggest further investigation. However, to dismiss one approach without any real scientific basis or proof is poor and should be avoided at all costs. Also, as both methods are empirical I find it difficult to see how you can state that one method definitively shows the other to be outside physical bounds. There is physical understanding but limited physics in either approach. [Peter Thorne]	
3-1032	A	29:9	29:11	Though the IPCC authors may believe utilizing all MSU overlaps, as does RSS, is preferable, they have not seen the actual quality of those small overlaps (alluded to in the appendix). The amount of error these small overlaps can introduce is larger than their period of length justifies. We tested all of these overlaps, large and small and It was the instability of these small overlaps that led us (Christy et al. 1995 and later) to not include them. Indeed this is likely the reason for an apparent spurious jump in RSS in 1991/1992 compared with several other datasets (a subject dealt with in the paper to be forwarded to the IPCC authors.) Too, for T2LT, there is no real difference between UAH and RSS prior to 1992 (tropical trends are –0.084 RSS and –0.064 UAH 1979-1991). The main difference is due to the NOAA-11 and NOAA-12 merge in Dec 1991 which has some dependence on a short NOAA-10 - NOAA-12 overlap used by RSS but not by UAH. UAH’s transition is based on a backbone of NOAA-11 at this point, not on the convoluted relationships of NOAA-10, -11 and –12.	

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				[John R Christy]	
3-1033	A	29:15	29:17	The statement is accurate. The similar trend difference (~0.1 K decade-1) also occurred in T4. [Qiang Fu]	
3-1034	A	29:19	29:25	The issue of Vinnikov and Grody is a bit sensitive. These authors have been extremely reluctant to provide their data (they refused to allow it to be entered in the NOAA 2004 annual climate assessment) and have informed those of us who have seen the global and tropical averages that we cannot use them in publications. They refuse to release gridpoint data. Thus, they have prevented their data from rigorous comparison studies and in my opinion have taken themselves out of the scientific process. You may have described the technical situation as best as possible here, though VG have another version (VG3?) now being reviewed. It may be important to let you know that VG have never quantified their error estimates (i.e. intersatellite differences remaining after the adjustments) even after repeated requests for them to do so. RSS and UAH have always included such error values in their papers, with UAH going a step further by including the reduction of error in the trend difference found in the intersatellite overlaps (Christy et al. 2003). For UAH's perspective, this is an important statistic as it lets the readers now how successful the merging procedure is regarding trends. [John R Christy]	
3-1035	A	29:20		correct: "create" to "creates" [Hartmut Grassl]	
3-1036	A	29:21	29:23	Remove the following words, which are erroneous and misleading, "although they did not account for temporal variations in calibration target temperatures on individual satellites during overlap periods." Grody et al. (2004) documented scientific, engineering and statistical basis of their technique and provided a detailed explanation of why inter-satellite calibration cannot be based on temporal variations of observed brightness temperature and warm target temperature for the overlapping periods. The way you phrase it implies that they did not address the problem, when in actuality they used a different technique, which is more accurate. They certainly did account for for temporal variations in calibration target temperatures on individual satellites during overlap periods. [Konstantin Vinnikov]	
3-1037	A	29:24	29:25	Remove the following totally incorrect and misleading statement: " The VG2 method does not, however, fully address the correction for diurnal drift and cannot distinguish between land and ocean." In actuality, the VG2 technique does fully address the correction for diurnal drift and does distinguish between land and ocean. The technique for calibration of multisatellite observations by Grody et al. (2004) corrects observed	

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				microwave brightness temperatures but does not change the local time of each measurement. Vinnikov et al. (2004) developed a special statistical technique to analyze the diurnal and seasonal cycles in climatic records with arbitrary observation times. Vinnikov et al. (2005) used this technique to obtain homogeneous time series of globally, zonally, and regionally (including oceanic and land only regions separately) averaged brightness temperature and to estimate climatic trends. (Konstantin Y. Vinnikov, Alan Robock, Norman C. Grody, Alan Basist, 2004: Analysis of Diurnal and Seasonal Cycles and Trends in Climatic Records with Arbitrary Observation Times. Geophysical Research Letters, v. 31, L06205, doi: 10.1029/2003GL019196.) [Konstantin Vinnikov]	
3-1038	A	29:25	29:25	Please add a reference to the latest results, presented by Vinnikov et al. (2005). I think their figure 8 would make an excellent figure for the IPCC report. ref: Vinnikov, Konstantin Y., Norman C. Grody, Alan Robock, Ronald J. Stouffer, Philip D. Jones, and Mitchell D. Goldberg, 2005: Temperature trends at the surface and in the troposphere. J. Geophys. Res., in press. [Alan Robock]	
3-1039	A	29:25	29:25	Please add a reference to the latest results, presented by Vinnikov et al. (2005). I think their figure 8 would make an excellent figure for the IPCC report. ref: Vinnikov, Konstantin Y., Norman C. Grody, Alan Robock, Ronald J. Stouffer, Philip D. Jones, and Mitchell D. Goldberg, 2005: Temperature trends at the surface and in the troposphere. J. Geophys. Res., in press. [Alan Robock]	
3-1040	A	29:25	29:25	Please add a reference to the latest results, presented by Vinnikov et al. (2005). I think their figure 8 would make an excellent figure for the IPCC report. ref: Vinnikov, Konstantin Y., Norman C. Grody, Alan Robock, Ronald J. Stouffer, Philip D. Jones, and Mitchell D. Goldberg, 2005: Temperature trends at the surface and in the troposphere. J. Geophys. Res., in press. [Alan Robock]	
3-1041	A	29:27	29:40	This is an important paragraph that presents a concise assessment of recent important findings. Spencer at al. recently raised issues on the retrieval of tropospheric temperature trends from MSU channels 2 and 4. We (Johanson and Fu 2005) find that our derived weights are within about 10% of those obtained by Fu et al. irrespective of the choice of analysis period or training dataset. The trend errors in the retrieved global mean tropospheric temperatures tested using two independent radiosonde datasets are less than about 0.01 K/decade for all time periods of 25 years or longer with different starting and ending years during 1958-2004. The strong apparent sensitivity of the weights reported	

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				by Spencer et al. is a refelction of the inconsistencies between the different datasets used in their regression, which has no bearing on the robustness of the Fu et al. retrieval algorithm. [Qiang Fu]	
3-1042	A	29:30	29:40	This passage presents too rosy a picture of the Fu et al. approach and should be recast as part of the subsequent discussion of 2LT. The two are complimentary in many aspects but are treated here almost as competing mechanisms. This seems rather pointless. It is required to make clear that the presence of negative weights in the averaging kernel is frowned upon by the satellite community, or at least a large part thereof, and that Fu et al. does not create new information per se - simply another weighting function (and one with some undesirable characteristics from a purely theoretical stand-point) that gives no extra information than the two channels it is derived from. It cannot magically produce new information content from thin air, and this needs to be made much clearer than the present discussion acheives. [Peter Thorne]	
3-1043	A	29:30		Replace "low relative to those in the troposphere" by "underestimates of tropospheric warming". [Adrian Simmons]	
3-1044	A	29:35	29:35	superscript "1" after "decade" should be "-1" [Richard Allan]	
3-1045	A	29:35		I do not understand what "leaving a residual influence of less than +-0.01K" means. [Adrian Simmons]	
3-1046	A	29:38	29:38	Add "Kiehl et al. 2005" (J. Climate, 18, 2533-2539) after "Gillett et al. 2004". [Qiang Fu]	
3-1047	A	29:38	29:38	"although biases in depicting stratospheric cooling can affect results". I don't understand what is meant here - does this mean that the model used in Gillett et al. (2004) has biased stratospheric cooling and therefore the results are unreliable? More explanation is needed here, or a reference. I think as long as the T4 and T2 trends incorporate the same estimates of stratospheric trends, any bias in those trends should not effect the reconstructed tropospheric trends. Clearly if T4 is effected by some bias which doesn't influence T2 then this could be a problem, but I haven't heard any suggestion that this could be the case. [Nathan Gillett]	
3-1048	A	29:42	29:57	The revised UAH T2LT is still problematic based on our analyses, especially over the tropical land region. The RSS T2LT also has strange behavior over the SH extra-tropical land region. The large surface contribution is a disadvantage of T2LT, which requires a more accurate diurnal correction scheme. In addition, the effect of changes in surface	

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				emissivity may also become important in T2LT. Note that in Fig.3.4.3, the UAH T2LT (revised) trend in the tropics is still significantly smaller than those both in the troposphere using T2 and T4 and at the surface. [Qiang Fu]	
3-1049	A	29:42	29:56	See above comment re UAH temperatures [M James Salinger]	
3-1050	A	29:42	30:10	Santer et al Science 2005 shows that T2LT and Tfu are much smaller for UAH and the radiosondes than they are for RSS and only RSS measures have similar lapse rate trends to those seen in models. Chap3 contrasts with Chap 9 (page 41 lines 9-10) which concludes that simulations of differential warming rates between the surface and the free atmosphere are inconsistent with some observational records. The summary statement to chap 3 brushes over these remaining inconsistencies somewhat. [Peter Stott]	
3-1051	A	29:47	29:49	This is quite misleading. The basic temperature of UAH MSU is a microwave product, not a radiosonde product. Thus, it will show the annual cycle of the microwave brightness temperatures as it should. However, when it comes to temperature anomalies of the troposphere, as demonstrated in past examples drawn from Christy and Norris 2004, the two are interchangeable (same amplitude, same variation, correlation above 0.95) even in the 10 Antarctic stations along the coast where sea ice varies. [John R Christy]	
3-1052	A	29:49	29:51	The UAH LT v5.2 is consistent with simple retrievals of Fu. In the global 1979-2004 trend, UAH +0.115 K/decade, Fu-calculated is +0.115 K/decade (from CCSP) … not much more consistency than that. It is important to note here that any retrieval is no better than its antecedent data. Thus error ranges on T4 (about ±0.1 K/decade) and T2 (about ± 0.05 K/decade) lead to at least that much error in the retrieval. Also note that Spencer, R.W., JR Christy, W.D. Braswell and W.B. Norris, 2005: On the estimation of tropospheric temperature form MSU channels 2 and 4, JTech (in press) discusses the significant issues with simple retrievals such as Fu's and that they cannot remove stratospheric influence … it is impossible. Non-stationarity is also a key weakness (i.e. the statistics are not perfectly stable over time, thus contaminating the retrieval.) [John R Christy]	
3-1053	A	29:49	29:50	After "the UAH record," add "and they showed that the large UAH T2LT trend bias is largely atributed to the periods when the satellite had large local equator crossing time drifts that cause large changes in calibration target temperatures and large diurnal drifts.". [Qiang Fu]	
3-1054	A	29:51	29:57	Here again, the story is misleading. The old UAH diurnal cycle adjustment was of the wrong sign, but only in the tropics, not elsewhere (due to the timing of the point at which	

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				the satellite views the diurnal cycle.) The difference between RSS and UAH for T2LT is 0.07 K/decade, not 0.2 K/decade (line 57). I think it quite important that the readers know what the impact of this old diurnal cycle was; it was a warming of the UAH trend of only +0.035 K/decade, which was within our published error margin of ±0.05 K/decade (Christy et al. 2003). The reader is left to guess what the impact was as it is now written. [John R Christy]	
3-1055	A	29:51	29:51	Remove "and" before "Mears". [Qiang Fu]	
3-1056	A	29:54	29:54	Personal communication is not an acceptable reference for an IPCC assessment. It must be repleaced with a published source or the material based on the communication deleted.. [Lenny Bernstein]	
3-1057	A	29:54		Page 3.29 refers to "personal communication," which isn't permitted. [Melinda Marquis]	
3-1058	A	29:57	29:57	Is this the corrected or uncorrected UAH 2LT ? [Peter Stott]	
3-1059	A	30:0		By page 3-30 I had forgotten what some of the acronyms meant: UHI, DTR etc. Is there going to be an overall glossary of acronyms that can be quickly referred to? [Harry Bryden]	
3-1061	A	30:2	30:10	Again, the spurious shifts of both day and night sonde observations were addressed in Christy and Norris 2004. It is Sherwood et al. who did not fully address the appropriate temperature shifts. This the assumption contained in this paragraph (i.e. that Christy and Norris did not treat day-time cooling trends) is incorrect. Indeed we found most instrumentation shifts did indeed cause spurious cooling and needed to be “warmed up” (Christy and Norris 2004, their Table 1). I believe it was a serious oversight on the part of Sherwood et al., not to reference our (and others) previous work in this area. [John R Christy]	
3-1062	A	30:2	30:10	As per an earlier comment, much of this discussion is unfair on the radiosonde datasets. Many of the comparisons referred to are *raw* radiosondes vs. satellites so reflect particularly badly on radiosondes. Of the studies cited in this paragraph only Randel and Wu consider a homogenised radiosonde dataset. It is important to recognise that many of the problems in the radiosonde data in these comparisons may have been removed in the climate-quality datasets that are described elsewhere within the chapter. [Peter Thorne]	
3-1063	A	30:4	30:4	"and a little less than RSS trends" is not accurate. The T2 trends cited in Christy and Norris (2004) are 0.12 and 0.03 from RSS and UAH, respectively. They disputed the RSS and VG T2 analyses by showing the "excellent" agreement between UAH T2LT and radiosonde data.	

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				[Qiang Fu]	
3-1064	A	30:6	30:10	Sherwood et al should be in 3.4.1.1 radiosondes, rather than here? [Neville Nicholls]	
3-1065	A	30:10	30:10	Change “this too is likely.” with “ this is likely (very likely)”. [Aristita Busuioc]	
3-1066	A	30:10		Here, and elsewhere, reference is made to "changes in sondes". Some of the problems are associated with changes in sondes, i.e. the instruments, but others are associated with changes in the processing applied by the sonde operators. A particular example is the US operated sondes. For many years data from these were not subject to a radiation correction f+G43or daytime ascents; when a correction was introduced, this in turn introduced an artificial shift in the time series. So "changes in sondes" should be "changes in sondes and the processing of data from them by station operators" [Adrian Simmons]	
3-1067	A	30:13	30:52	ERA-40 contains so many residual problems in the data for upper-air temperatures, particularly within the tropics, that I find it incredible that inclusion of this section is even being considered. Use of reanalyses to characterise vertical temperature trends was rejected unanimously by the CCSP panel instigated to consider this topic as they were felt by this expert group to be so obviously inadequate. Caveats that should ring alarm bells about the vertical coherency of the signal and cancellation are buried so deep in the text as to be misleading. I don't know how much more clear I can be: ERA-40 is inadequate for characterising long-term trends and channel 2 agreement is hugely fortuitous and should not be used to suggest any skill whatsoever. Regardless, being a named author on both these competing viewpoints would put me and others in a similar position in an impossible situation if this section were to be retained in its current form. [Peter Thorne]	
3-1068	A	30:14	30:53	Pardon the continuing diatribe, but these data sets have been seriously pathological relative to what we need from them for climate monitoring. See NASA's inattention, NOAA's poverty, and WMO's dysfunction for insight on what needs to be done globally to crack this problem. [Jerry Mahlman]	
3-1069	A	30:15		Give a Table of results of reanalysis [Vincent Gray]	
3-1070	A	30:17		Change "operational global analyses to "global analyses from operational NWP". [Adrian Simmons]	
3-1071	A	30:22	30:24	The statement is not supported by Fig. 3.4.3. [Qiang Fu]	

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3-1072	A	30:22	30:24	Comment: Figure 3.4.3 (bottom) shows tropical surface air temperature trends from both reanalyses appearing to be smaller (though the error bars are quite large) than other estimates. Does this feature deserve some mention in the discussion, especially in the context of the comment on the "generally good agreement" with Jones and Moberg (2003)? [Thomas Knutson]	
3-1073	A	30:28	30:40	It's not clear if the ERA-40 takes into account the effect of the hot target temperature variations in the calibration. [Qiang Fu]	
3-1074	A	30:33	30:34	Incorrect. UAH and ERA-40 are closer for T2 and T2LT than is RSS (see your Fig. 3.4.3). However, the authors' next statement is more important and is correct, that ERA-40 above 200 hPa is clearly in error for trends since 1979 (spuriously warm), and thus suggesting any vertically integrated product involving stratospheric layers to be suspicious. [John R Christy]	
3-1075	A	30:39	30:39	Change "almost certainly" with "virtually certain" [Aristita Busuioc]	
3-1076	A	30:42	30:52	This paragraph has been carelessly and incorrectly edited from the text originally supplied by the ERA-40 team. ERA-40 ran only to August 2002, so the reference to 1979-2004 is demonstrably incorrect. The figure (3.4.4) applies to the 1979-2004 period, and includes a correction due to Fu. The text refers to a published comparison (Santer et al., 2004) which does not include the correction due to Fu. The text needs correcting, and the reference to Santer et al. moved to the first sentence, as both the tropospheric and stratospheric remarks are related to that paper. Either drop the figure, or replace it with a figure derived from Santer et al that at least shows both ERA-40 and the RSS maps (Figs 11a and 11b of the paper). Santer can, I'm sure, provide the data for replotting if the colour scheme is deemed unacceptable. [Adrian Simmons]	
3-1077	A	30:47	30:49	Again a misunderstanding. MSU datasets do not "treat" surface emissivity, it is part of the product. Swanson 2003 was challenged to support his claim of contamination concerning temperature ANOMALIES and could not respond to the challenge because the evidence supported the radiosonde agreement with UAH anomalies. I think one could just eliminate the Swanson paper as there is no information content not already known (i.e. we've always known that absolute MSU brightness temp is just that and is affected by surface emissivity variations… Spencer et al. 1990) [John R Christy]	
3-1078	A	30:50	30:52	The Bromwich and Fogt (2004) reference does not align with the text here. Although	

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				these lines are true, there really isn't a published source to our knowledge to refer to them. However, a recent publication by Turner et al. (2005) shows temperature trends resolved by the radiosonde and surface network around Antarctica, but misses the strong trends seen over West Antarctica (see Fig. 3.5.1) do to data voids. Additionally, both our research and that published by Turner et al. (2005) demonstrates that these trends are strongly seasonal dependent (with some reversal between austral summer and winter) and exist throughout the troposphere and into lower stratosphere. [David / Ryan Bromwich / Fogt]	
3-1079	A	31:0		It is discouraging to see the now-identified problems with monitoring temperature trends in the lower stratosphere. However, the good news is that the measurement of stratospheric T trends have evolved to semi-quantitative status, with the updated trends being consistent with previous theoretical calculation of cooling trends that will occur well into the mesosphere. [Jerry Mahlman]	
3-1080	A	31:0		Section 3.4.1.4: does the consistency between the tropopause height and the tropospheric temperature depends on the latitude (tropics and mid-latitudes)? [Paolo Michele Ruti]	
3-1081	A	31:1	31:18	How the tropopause position was calculated? What is the precision of its definition? [Eugene Rozanov]	
3-1082	A	31:1		Section 3.4.1.4 This does not concern direct observations and is covered in Chapter 9 in the context of D&A. It is outside the remit of this chapter and hence should be deleted. [Peter Thorne]	
3-1083	A	31:2		Add "generally" between "and" and "a". The temperature of the polar winter tropopause is often not a minimum. [Adrian Simmons]	
3-1084	A	31:7	31:8	Where did the idea come from that the tropopause height "provides a sensitive indicator of human effect on the climate" come from? It seems that while it may prove to be an indicator of changes in the temperatures above and below it, it is not particularly any more sensitive to human effects than non-human effects. Plus, this is a Chapter on Observations, not an attempt to link the observations to human activities. That should be left to the appropriate chapter. [Jeffrey Kueter]	
3-1085	A	31:10	31:18	Is it possible to produce a line diagram showing trends and variability in globally averaged tropopause height?? This would be a very interesting global giagnostic suggestive of various mechanisms of variability and change. [M James Salinger]	
3-1086	A	31:21	31:37	Much of the detail in this paragraph should be moved to the figure caption.	

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				[Melissa Free]	
3-1087	A	31:21	32:43	Unfortunately this section is extremely poorly written and involves completely inappropriate and meaningless use of statistics to make incorrect conclusions regarding dataset consistency both within and between levels. Trend uncertainties are the wrong statistics to show. They reflect the spread about a line of best fit, which, given the small trend and high variance is dominated by the high frequency variance which is common to all datasets. The statistics to show are either structural and parametric (published error estimates of dataset construction), or the uncertainty in difference series. These then show a less rosy picture with little if any overlap (certainly not significant) and point towards residual differences between datasets. See the CCSP appendix for a discussion of these aspects and a very striking example of why the SEs should not be used in the manner that they currently are here. What is there at the moment is simply a jumbled white-wash to imply everything is okay. I wish it were and that we could get on with more interesting, relevant, science, but it isn't and it's important not to gloss over this fact. [Peter Thorne]	
3-1088	A	31:28		"some discrepancies arise" is too vague. How about "so some spatial sampling error exists". [Melissa Free]	
3-1089	A	31:30	31:32	The error bar question is a tough one (see also my comments under Fig. 3.4.3). My preference is to deal only with measurement error - that is what the audience assumes when they see an error bar. Measurement error comes from spatial subsampling and structural issues, not from the specific time period being examined. The UAH published error bars (based on comparisons with groups of independent data) are ±0.05 K/decade for global values. The statistical sampling error bars makes everything look like it does not disagree with everything else. The real test is to do the comparisons with difference times series (such as in the paper forthcoming.) [John R Christy]	
3-1090	A	31:32	31:33	"incomplete spatial sampling may increase the variance but also increases the noise". Aren't these the same thing? [Nathan Gillett]	
3-1091	A	31:36	31:37	This sentence is unclear. [Melissa Free]	
3-1092	A	31:39	31:46	This description is very poor. The similarity in the middle of the period is because they have all been anomalised relative to this period, so they are all going to be close to zero at this time - similarity is simply a statistical artefact. The discussion of the figure needs significant improvement to be worth retaining. It needs to point out that there is strong agreement over timing and amplitude of higher frequency events and the disagreement	

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				occurs at low frequencies. This is indicative of the presence of residual inhomogeneities (or red noise) in at least some and probably all of the datasets. These problems will not be evident on high frequencies. This message is the one that should be being communicated with regard to this figure. [Peter Thorne]	
3-1093	A	31:41		Insert after "1970s". "The rise in the surface record took place over the entire 1979-2004 period whereas the rise shown by the MSU and radiosondes took place exclusively after 1997, with a very large influence from the 1998 El Niño event. The MSU showed no rise in temperature at all between 1979 and 1997.in contrst to the surface record." [Vincent Gray]	
3-1094	A	31:41		Move "From in the lower stratosphere" to form a new paragraph [Vincent Gray]	
3-1095	A	31:48	31:51	If linear fit is not good, what metric or approach can be recommended? [Eugene Rozanov]	
3-1096	A	31:49	31:50	Indeed, so not only linear but also non-linear trends should be given if it is appropriate (but not trends based on filtered data, see remark 11 above). [Christian-D. Schoenwiese]	
3-1097	A	31:50	31:51	Does ENSO really affect stratospheric temperatures? Perhaps it would be better to argue it's the QBO? Or to make clear that the troposphere is being talked about in this second sentence. Even better would be the removal of these inappropriate error statistics and associated discussion. [Peter Thorne]	
3-1098	A	31:51	31:51	A linear fit for dTa/dt is not so good; how about dTa/dTs? (where Ta is atmospheric temperature) - this can show how strongly the atmospheric temperature is coupled to surface temperature which is physically meaningful in the tropics. [Richard Allan]	
3-1099	A	31:51	31:51	Is this referring to the stratosphere, and do you mean QBO? Or do you mean the troposphere? [Melissa Free]	
3-1100	A	31:53	32:18	It is a nice and important summary and explanation for Fig. 3.4.3! [Qiang Fu]	
3-1101	A	31:54	31:54	insert: "appear to" between 'radiosondes' and 'still' -- This is not a certainty. [Melissa Free]	
3-1102	A	31:54	31:54	Radiosondes *very likely* overestimate. No-one has proven this is the case for the homogenised data beyond a few tropical stations in RATPAC, which makes a very definitive statement unwise. Until it is proven for other homogenisation techniques it is	

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				better to argue that it is very likely to apply to these. Statements of fact without the pre-requisite proof are essentially meaningless and very poor science. [Peter Thorne]	
3-1103	A	31:55		Add " and changes in processing such as radiation corrections" after "instrument changes". [Adrian Simmons]	
3-1104	A	31:56		There is no specific support connecting the Randel or Sherwood effects to balloon burst issues. [Melissa Free]	
3-1105	A	31:57	31:57	Change “obvious” with “very likely (virtually certain)”: what it is most appropriate according to the Uncertainty Guidance Note; I think that “virtually certain” is better [Aristita Busuioc]	
3-1106	A	32:0		Another new technology based on GPS, ground-based GPS for determining precipitable water vapor, should be mentioned in section 3.4.2-Water Vapour. An early reference, in addition to the Businger et al. (1996) reference above is: Bevis, M., S. Businger, S. Chiswell, T. A. Herring, R.A. Anthes, C. Rocken, and R. H. Ware, 1994: GPS Meteorology: Mapping zenith wet delays onto precipitable water. Journal of Applied Meteorology, 33, No. 3, March 1994, 379-386. [Richard Anthes]	
3-1107	A	32:2	32:3	Important to make clear that this is the case whether you remove statistically or by the physical limb differencing approach (following identification and adjustment for a previous error in UAH). [Peter Thorne]	
3-1108	A	32:3	32:5	I don't think this adds any useful information and would strongly advocate its deletion. [Peter Thorne]	
3-1109	A	32:6	32:8	The material about NRA and greenhouse forcing should be dealt with in the sections about reanalyses, not raised here for the first time. [Melissa Free]	
3-1110	A	32:8	32:10	How big is this effect? My understanding was that it is small. [Nathan Gillett]	
3-1111	A	32:8	32:10	This sentence has no useful content as we are discussing microwave retrievals so changes in the infra-red are entirely an academic matter with regard to our ability to recreate faithfully past climate changes. [Peter Thorne]	
3-1112	A	32:10	32:18	The small change for the mid- to lower troposphere temperature might be true, and the	

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				inhomogeneity of radiosonde data is not so serious at least in China. For the period of 1979-2004, no decrease in annual mean temperature in the mid- to lower troposphere has been found. Rather a significant rising in temperature in these layers has been detected (Wang, Y. and Ren, G. Y., 2005, Change in free atmospheric temperature over China during 1961-2004, Climate and Environmental Research, 10 (4) (in press in Chinese)). [Guoyu REN]	
3-1113	A	32:10	32:11	The records only suggest this if a flawed liberal indicator of uncertainty is used so this needs to be modified after taking account of errors properly as detailed elsewhere in this review. [Peter Thorne]	
3-1114	A	32:16	32:16	It was the radiosondes and UAH - so only RSS agreed with models. Statement needs correcting to make this clear. [Peter Thorne]	
3-1115	A	32:16	32:18	There is a mis-interpretation here that Sherwood et al corrected for day - night. They parameterised the likely affect, that's all. They also, as they state, did not account for the large number of other known problems in the data and these may magnify, ameliorate or make no difference to their implied adjustments required. This sentence requires redrafting to reflect the above. [Peter Thorne]	
3-1116	A	32:17	32:17	Soften this conclusion--"they too would probably show…" [Melissa Free]	
3-1117	A	32:17	32:18	Sherwood (2005) does not produce a homogenised data set of radiosondes - the day time biases are only one component of a complex problem - I don't think this statement is defensible as written. [Mark McCarthy]	
3-1118	A	32:17	32:17	"... They too show ..." not clear; typo? [Christian-D. Schoenwiese]	
3-1119	A	32:20		Insert a Table showing regional temperature trends from surface and MSU measurements [Vincent Gray]	
3-1120	A	32:29	32:33	Figure 3.4.2 does not give the impression that surface and tropospheric temperature trends agree. Surface temperature increases during the 1980s-90s but the same isn't so obvious for the troposphere. It would be useful to plot the troposphere minus surface temperature record and refer to this in the text. The 1976 "climate shift" has never been adequately explained; this would appear more important than analysing trend differences. [Richard Allan]	
3-1121	A	32:29	32:43	The bottom line, which appears to be largely missing or interpreted in a non-even handed	

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				way, is that the evidence is that the troposphere at almost all levels, except perhaps close to the stratosphere, is warming globally and likely in the tropics too. Thus the conclusion of the TAR that the tropical troposphere is not warming or is cooling is now (very) unlikely to be true. However globally or in the tropics we are still unsure whether the mid or lower tropospheric warming trends since 1979 are greater, equal to or less than the surface trends. This is because of the large structural uncertainty in the satellite data and the even greater difficulties of the radiosonde data which have not been resolved, though progess has been made since the TAR. Further work is needed to narrow the fairly substantial remaining uncertainties. [Chris Folland]	
3-1122	A	32:29	32:43	Begin the paragraph with the caveats about linear trend fits, then move on to discuss the trends? [Melissa Free]	
3-1123	A	32:29	32:35	This gives the impression (wrongly) that radiosondes and surface agree despite the caveats. Its not possible to diss the radiosonde records 1979 to date and laud them over 1958 to date because *half the data in the longer period is common to the sub-period*. Sanity says that any such two statements are entirely contradictory no matter how much they are caveated. Critics of the process and climate science will find this contradiction such an easy target that retention simply makes the chapter a hostage to fortune. [Peter Thorne]	
3-1124	A	32:29		Replace “in close accord with” with” “somewhat lower than” [Vincent Gray]	
3-1125	A	32:33	32:34	This statement is ambiguous--how "similar" are they? Half the datasets (UAH, sondes) show significantly less warming in the Tropics than models indicate should accompany the reported surface warming. This sentence implies consistency of all datasets. What it should convey is that the spread of trends among tropospheric datasets encompasses that which would be consistent with the surface warming. This comment also applies to 3-79:31-37. [Steven Sherwood]	
3-1126	A	32:34	32:36	It is stated here that since 1979 “trends are greater for the surface over land…where minimum temperatures have risen more than maxima.” Yet, back on page 15, lines 1-2, it states” Figure 3.2.11 shows annual trends in DTR over 1979-2004. As noted in Figure 3.2.2, for this period, DTR trends overall are small and depend a lot on starting and ending years.” So there is no widespread evidence of DTR changes since 1979. This is echoed in the Executive summary which states (page 3, line 24 DTR “had virtually zero change from 1979-2004.”	

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				[Jeffrey Kueter]	
3-1127	A	32:34		Insert after "estimates". "but all the increase in the troposphere took place after 1997 because of the very large effect of the 1998 El Niño event." [Vincent Gray]	
3-1128	A	32:34		Delete "although and a capital letter for "Trends" [Vincent Gray]	
3-1129	A	32:36		Start a new paragraph with "Clarification" [Vincent Gray]	
3-1130	A	32:38	32:38	It would be informative to explicitly state that the previous temperature records have a spurious cooling contribution from the stratosphere (Fu et al. 2004a) [Richard Allan]	
3-1131	A	32:40	32:43	Delete from "It is apparent" in line 40 to "cooling" in line 43. It merely repeats what has already been overdone. [Vincent Gray]	
3-1132	A	32:42	32:43	This sentence should be deleted as there is no purely observational evidence to support it and cause and effect is the domain of Chapter 9. Requires discussion with chapter 9. [Peter Thorne]	
3-1133	A	32:44		Insert a new Section dealing with proxy temperature measurements since 1900. They should not be relegated to the "Paleo" Chapter as they enable a judgement of the anthropogenic effects of the surroundings of weather stations compared with the lesser human influence of the proxies. [Vincent Gray]	
3-1134	A	32:45	33:4	Mention that surface humidity data are not very accurate because the wet and dry bulbs are not usually aspirated. Regional trends in wind speed (over land or ocean), ships speed or thermometer screen design changes could thus cause artificial humidity trends. Such potential problems have yet to be assessed and corrected for. [Chris Folland]	
3-1135	A	32:45	37:9	Water Vapour is a greenhouse gas. This whole section should be transferred to Chapter 2 [Vincent Gray]	
3-1136	A	32:47	32:51	I have some real problems with the unqualified assertion that water vapour is the "most important greenhouse gas". Even in relatively simple model calculations, adding water vapour to,say, 2xH2O levels, produces a strong short warming forcing, but most of the added water vapour is gone in a month or so. Double the CO2 concentratons, with its 1/e uptake time of centuries, and the CO2 just stays there. Clearly, such simple statements are attractive, but are a bit too subtle for soundbite statements. Subtilties are involved here in this question, but it is important to avoid sound bites about "the most important	

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				greenhouse gas being water vapour" that has been so deliberately misused by the contrarians. Indeed, it is almost an urban legend! [Jerry Mahlman]	
3-1137	A	32:54	32:55	replace the part of sentence ",but it is only…data from this time" by: "but there have been no attempts so far comparable to those for temperature and precipitation to construct global homogenised long-term humidity datasets." [Reinhard Böhm]	
3-1138	A	32:55	32:57	This sentence (However, the surface water vapour …) is vague and doesn't convey much information to me. [Dian Seidel]	
3-1139	A	32:55		The phrase "principally due to international exchange of data from this time" is a bit strange. The network can be considered good for earlier times for climate purposes despite the lack of near-real-time international exchange provided nations have not thrown away the older data. Data recovery may require much more effort, but this does not mean there is not a network of observations. [Adrian Simmons]	
3-1140	A	33:0	34:	Section 3.4.2.1 - 3.4.2.2: There is repetition between Section 3.4.2.1 and 3.4.2.2. I suggest combining the two sections. For example, remove 1st paragraph of Section 3.4.2.1 apart from the sentence beginning "Increasing extremes in summer…" which can be appended to the 1st para of 3.4.2.2. The 2nd para of 3.4.2.1 could become the 2nd para of 3.4.2.2 and the summary of 3.4.2.1 combined with the summary of 3.4.2.2 [Richard Allan]	
3-1141	A	33:0	34:0	Again, measures of water vapour have been described in loose, non- quantitative language. I suggest "mixing ratio", a robust measure, or "mole fraction" to please the chemists. [Jerry Mahlman]	
3-1142	A	33:17		Section 3.4.2.1 I suggest that "Near-surface" be used instead of "surface." [Steven Sherwood]	
3-1143	A	33:18	33:34	Please insert (with possible editing) in this paragraph: Potential forest fire indices used in Russia (Zhdanko and Nesterov) are based on accumulated surface water vapour deficit. Groisman et al. (2005b) show a systematic increase in these indices during the 1936-2000 over the Asian part of Russia and thus an increase in atmospheric spring and summer dryness over this part of the world. [Pavel Groisman]	
3-1144	A	33:18	33:47	Here the text acknowledges that there is a positive upswing in actual vapor pressure (which tends to reduce VPD), a quantity considered stationary in the PDSI.	

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				[Michael Hobbins]	
3-1146	A	33:30	33:30	after "…China reported in the TAR." please add: "For Central Europe Auer et al., 2005b confirm the general increasing moisture trends. Their vapour pressure series from the Greater Alpine Region follow closely the decadal and centennial warming in the region in all kind of sites from lowland-urban to rural-summit sites up to more than 3000m asl." [Reinhard Böhm]	
3-1147	A	33:30	33:30	Add the following text: " In Canada, Van Wijngaarden and Vincent (2005) found that an apparent strong decrease in relative humidity in winter since 1953 was due primarily to the change from psychrometers to dewcels in the early 1970s. After accounting for the effects of this change, data from 75 Canadian stations show a relative humidity decrease of several percent in the spring in western Canada, but little change elsewhere or for other seasons." [Melissa Free]	
3-1148	A	33:30	33:30	Delete the sentence beginning "Reported trends…" since the Ishii and Dai studies are not so limited. [Melissa Free]	
3-1149	A	33:35	33:43	Suggest to replace the whole paragraph with the following: In an attempt to update near global (60deg.S-75deg.N) surface humidity trends, Dai (2005) analyzed individual synoptic reports from over 15,000 land stations and ships from 1976 to 2005 for changes in surface specific and relative humidity and their relationship with temperature warming. The results show that from 1976-2004 global changes in surface relative humdity are small, although decreasing trends of ?-0.11 to ?-0.22% per decade for global oceans are statistically significant; whereas specific humidity has increased by 0.06 g/kg per decade globally and 0.08 g/kg per decade in the Northern Hemisphere. The humidity-temperature anomaly relationship is approximately linear so that surface specific humidity over the globe, global land and ocean increases by ~4.9%, 4.3% and 5.7% per 1deg.C warming, respectively, which are close to those suggested by the Clausius-Clapeyron equation with a constant relative humidity. [Aiguo Dai]	
3-1150	A	33:35	33:35	Dai (2005) appears to be missing in the refs. A global specific humidity trend map for 1976-2005(?) based on Dai's results with significant areas highlighted would be good. [Chris Folland]	
3-1151	A	33:38	33:39	I guess you mean that the the H2O mixing ratio tends to be conserved in the nightime boundary layer, assuming no condensation. [Jerry Mahlman]	
3-1152	A	33:50	33:57	Reiterates the sentences in 3.4.2.1	

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				[Guoyu REN]	
3-1153	A	33:50	34:2	I get a very strong sense of déjà vu from the text in the previous section (almost line for line). Needs correcting for. [Peter Thorne]	
3-1154	A	33:52	4:2	The surface water vapor material belongs in the previous section. Delete the text that duplicates material in section 3.4.2.1. Move the sentence beginning "Philipona and Durr…" to that section. [Melissa Free]	
3-1155	A	33:54	34:2	Repetition of page 33 lines 27-33 [Mark McCarthy]	
3-1156	A	33:56	33:56	The reference should be : Philipona et al., (2004) [Rolf Philipona]	
3-1157	A	33:57	33:57	Please correct : …over the period 1995-2002. Please add here the sentence below: [Rolf Philipona]	
3-1158	A	33:57	33:57	In a more extensive study contrasting radiative fluxes to the evolution of temperature and integrated water vapor over Europe (Philipona et al., 2005) they show that temperature changes are strongly coupled to changes of integrated water vapor, with rather decreasing values in the west but rapid increases in Central and Northeastern Europe. [Rolf Philipona]	
3-1159	A	33:57	33:57	The sentence strating from "Trend" appears earlier (line 31, same page) [Eugene Rozanov]	
3-1160	A	33:57		ESTIMATES OF trends in surface observations… [Neville Nicholls]	
3-1161	A	34:0		The text states that " upward water vapour trends are still almost limited to regions north of 20 deg N". Yet, it is said just earlier that ocean temperatures have increased by about 0.6deg C. in tropical regions, without any seeming expectations that it is really hard to increase tropical ocean temperatures without increasing tropical mixing ratio, at rougly conserved relative humidity. Please clarify. [Jerry Mahlman]	
3-1162	A	34:1	34:2	The Ishii et al (2005) sentence repeats lines 32-33 of p 33. [Chris Folland]	
3-1163	A	34:4	34:9	PW from SSM/I is due largely to the work of Frank Wentz and RSS. I do not see such a reference. [John R Christy]	
3-1164	A	34:7		Change "to provide" to "as providing". [Adrian Simmons]	

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3-1165	A	34:11		'confusuion' seems like a very value-laden word. Perhaps 'different findings' would be better. [Ian Simmonds]	
3-1166	A	34:16	34:18	The statement is not very clear and it is necessary to be reformulated; add the uncertainty related to the fact that the time series is too short [Aristita Busuioc]	
3-1167	A	34:18		The time period cited here seems to be inconsistent with the Exec Summy (see page 4, line 26) and with Fig 3.4.5? [Martin Manning]	
3-1168	A	34:20		The simple Clausius-Claperone equation has been explained earlier in this text without a needed recent reference. This is all common knowledge in widespread textbooks. [Jerry Mahlman]	
3-1169	A	34:21		Add "and Uppala et al.(2005)" after "Trenberth et al. (2005a)". [Adrian Simmons]	
3-1170	A	34:27	34:30	The purpose of this paragraph is unclear - I understand that it is as an assessment of ERA-40, rather than an assessment of the quality of the water vapour observations. Particularly as the next paragraph goes on to criticise the reanalyses. [Mark McCarthy]	
3-1171	A	34:32	34:43	The ERA40 data agree well with the NCEP/NCAR reanalysis data in the origins and transport of the water vapor associated with typical anomalous summer rainfall patterns of China, See: Zhou, T.-J., and R.-C. Yu (2005), Atmospheric water vapor transport associated with typical anomalous summer rainfall patterns in China, J. Geophys. Res., 110, D08104, doi:10.1029/2004JD005413. [Rucong Yu]	
3-1172	A	34:32	34:43	The ERA40 data agree well with the NCEP/NCAR reanalysis data in the origins and transport of the water vapor associated with typical anomalous summer rainfall patterns of China, although they are weaker than the NCEP/NCAR data at the lower atmosphere in terms of quantitative comparison. For reference, see: Zhou, T.-J., and R.-C. Yu (2005), Atmospheric water vapor transport associated with typical anomalous summer rainfall patterns in China, J. Geophys. Res., 110, D08104, doi:10.1029/2004JD005413. [Tianjun ZHOU]	
3-1173	A	34:32	34:44	The implied air-sea freshwater flux is examined in EAR15 and NCEP re-analyssis data. Not only for annual mean state but also for seasonal variation, two reanalysis agree qualitatively well with each other, and both can reproduce reasonably the global distribution of E-P (i.e. Evaporation minus Precipitation) over the ocean. In the view of quantitative comparison, however, remarkable difference has been found on regional scales, especially over the middle and lower latitudes, with some local disagreement	

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				exceeding 100 cm/yr. The re-analyses data-based estimations are less than COADS-based E-P flux. See reference: Zhou Tianjun, 2003, Comparison of the global air-sea freshwater exchange evaluated from independent datasets, Progress in Natural Science, 13(8), 626-631. [Tianjun ZHOU]	
3-1174	A	34:39	34:43	NRA column integrated water vapour shows good agreement with SSM/I for interannual variability but has a poor spatial ditribution and exhibits a negative trend in contrast to SSM/I and ERA40 data (e.g. Allan et al. 2004). Suggest: "However, interannual variability after the 1970s have been shown.....are unrealsitic (Trenberth 2005a) as are the spatial distributions and and trends over the tropical oceans (Allan et al. 2004)." [Richard Allan]	
3-1175	A	34:45	34:49	Though I'm only pointing out suggested improvements I want to pause here an complement the authors on this paragraph. This is precisely what PW represents. [John R Christy]	
3-1176	A	34:48	34:49	It might be more appropriate to refer to "some NH land areas" because the studies referenced do not sample all the northern hemisphere continents. [Mark McCarthy]	
3-1177	A	34:48	34:48	Nowhere in the preceding text could I find discussion of NH land so I'm perplexed that the summary is summarising data that is not even presented in the text being summarised. [Peter Thorne]	
3-1178	A	34:51	34:51	Figure 3.4.5. The top chart is very suspicious off the coast of Baja California. I would like a mechanism to explain a 6% drop in precipitable water there. Failing that, maybe the text should not be so confident about these data. Figure 3.2.9. certainly shows no similar bullseye. [Jeffrey Kueter]	
3-1179	A	34:51		There is no reference here in the text to fig 3.4.5. There is a reference much further on, on page 3-71. Why not adding a section on precipitable water? [Fons Baede]	
3-1180	A	34:54	35:40	The issue of whether the upper tropospheric water vapor feedback operates as expected is a key one that has been the focus of much attention in past IPCC reports. The new paper by Soden et al. (2005) seems to me to be very helpful in addressing this, and merits more discussion in this report. More discussion of this is needed in this section. In addition, I think it would be a loss to the report if Figure 3 of that paper did not appear in this AR4. Could an adaptated version of this figure be considered for section 3.9? While a model is used in the figure, the key point is a simple conceptual one and that seems to me appropriate for this section. or perhaps it could reside in chapter 2 or chapter 9? [Susan Solomon]	

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3-1181	A	34:56		The impact of upper troposphere water vapor was analyzed earlier by Spencer and Braswell: Roy W. Spencer and William D. Braswell. 1997: How Dry is the Tropical Free Troposphere? Implications for Global Warming Theory. Bulletin of the American Meteorological Society: Vol. 78, No. 6, pp. 1097–1106. [John R Christy]	
3-1182	A	35:0		Due to its critical importance to climate, water vapour observations in the upper troposphere and lower stratosphere need to be an integral part of future integrated observing systems for the climate. Perhaps words to this effect should be included. [Mark McCarthy]	
3-1183	A	35:2	35:9	I find this to be quite dubious. Also, what is "a reduction in moisture"? I think we need some cleaning up here. Based on the governing physics and transport that control atmospheric water vapour mixing ratios, this conclusion appears to be very logically inconsistent. [Jerry Mahlman]	
3-1184	A	35:8	35:9	This suggestion is unlikely to be correct, since Minschwaner and Dessler obtained independent information on relative and specific humidity from two different limb sounders, giving consistent results. If there is information to support this allegation in spite of the above, it needs a citation. On the other hand, the cited study did NOT find a trend in relative humidity, only a (weak) correlation in interannual variations. [Steven Sherwood]	
3-1185	A	35:20	35:31	As an informed reader, I think a little more information here could help. Tropics wide UTH may show one thing, but if separated by region (i.e. deep tropics with ITCZ vs. descent regions) something else may show up. Increases in UTH near the ITCZ will have little impact because those places are already moist, but changes in the dry regions are the key factor (Spencer and Braswell 1997). Any information on that would be appreciated. [John R Christy]	
3-1186	A	35:20	35:31	The equivalent humidity weighting profiles for the 6.3-6.7 water vapour channels (e.g. Soden and Bretherton 96) show they are sensitive to a broad atmospheric layer encompassing the mid to upper troposphere peaking near 300hPa. It would be useful for the reader to include more information on the instrument measurement. It is not strictly speaking upper tropospheric humidity, one might still argue that the HIRS platform is not sufficiently sensitive to changes in the tropical upper troposphere (say 200-100hPa) to give a complete assessment of upper tropospheric water vapour. [Mark McCarthy]	
3-1187	A	35:29	35:31	There remain considerable uncertainties in the temperature trends of the upper	

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				troposphere. Therefore there is no "observed" moistening of the upper troposphere as suggested in this sentence. Soden (2005) provides further evidence of constant relative humidity but no direct observations of increases in water vapour mixing ratio. [Mark McCarthy]	
3-1188	A	35:33	35:36	It should perhaps be mentioned that consistency of OLR changes with constant relative humidity tends to confirm positive water-vapor feedback or at least invalidates proposed negative water-vapor feedback in the upper troposphere. [Robert KANDEL]	
3-1189	A	35:33	35:36	This a good place to put bad water vapour measurements into a more physically based perspective. [Jerry Mahlman]	
3-1190	A	35:33	35:36	Allan et al. 2002, JGR 107(D7) 4329-4335 show that models with very different lapse rate and water vapour feedbacks produce very similar clear-sky OLR changes - presumably compensating errors of this type could also occur in trying to use OLR changes to confirm the relative humidity assumption. This is not a cast-iron validation of the temperature and water vapour observations and caveats to this effect should be included. [Mark McCarthy]	
3-1191	A	35:36	35:36	…and Slingo 2002) and changes in well mixed greenhouse gases (Allan et al. 2003). [Richard Allan]	
3-1192	A	35:38	35:40	From the available evidence I dispute that there is "widespread" evidence for global increases in specific humidity in the upper troposphere. The observations have not received the same level of scrutiny as the temperature records and the uncertainties are still poorly quantified. The lack of globally complete long-term records of upper tropospheric humidity is still a major constraint on our ability to validate simulations of recent climate change. [Mark McCarthy]	
3-1193	A	35:38	35:40	This summary paragraph is inconsistent with the section it is summarizing. The section suggests a lot more uncertainty than the summary paragraph. [Dian Seidel]	
3-1194	A	35:38		This seems to suggest that these data are sufficent to expect to detect a trend if one exists. Is this the implication the authors meant? [Neville Nicholls]	
3-1195	A	35:42	37:9	Section 3.4.2.4 seems to wander into process studies. Given the modest progress since the TAR, this section should be significantly shortened. [Chris Folland]	

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3-1196	A	35:42		Section 3.4.2.4: This is an example of a very well witten and readable section with a minimum of acronyms! [Fons Baede]	
3-1197	A	35:42		The discussion here (with caveats) addresses increases in stratospheric water vapor. This is occurring in an environment of significant cooling. On p. 3-25-56 it is stated that 'observations suggest that relative humidity remains about the same overall, and hence increased temperatures result in increased water vapor in the atmosphere'. To avoid giving the reader conflicting messages it may be worth stating here some of the arguments why the quasi-conservative nature of RH does not seem to hold in the stratosphere. (See also p. 3-43.) Such arguments are presented on page 3-36 (methane oxidation etc.), but it might be useful to 'get in early'.. [Ian Simmonds]	
3-1198	A	35:43	35:44	50% in 50 years? Is this a typo? [Michael Hobbins]	
3-1200	A	35:51	35:53	Satellite data interpretation has been given short shrift. Not only is it interesting that satellite data yields a substantially different trend than the Boulder balloon measurements, but curiously the two different instrumental time series seem to be well correlated over the past few years. This is very puzzling and inhibits reliable conclusions as yet about water vapor trend (if any) in the past decade. [V. Ramaswamy]	
3-1201	A	35:51	35:53	The statistical significance of these "discrepancies" was not established in that paper and, to my eye, they do not appear significant. [Steven Sherwood]	
3-1202	A	35:57	35:57	Regarding cooling of stratosphere: water vapor changes may yield a cooling comparable to CO2 (and well-mixed gases for that matter) in the lower stratosphere; however, it does not seem very likely that this would have been comparable to that due to lower stratospheric ozone loss. Even for the tropics, this may not be valid. [V. Ramaswamy]	
3-1203	A	35:57	35:57	Regarding warming of the surface, seems rather unlikely that water vapor effects could have been comparable to CO2 changes? [V. Ramaswamy]	
3-1204	A	36:0		Section 3.4.2.4 seems rather long. Specifically, paragraph 2 could be shortened to: "Although methane oxidation is a major source of water in the stratosphere, and has been increasing over the industrial period (Section 2.3.2), the noted stratospheric trend appears to be too large to attribute methane oxidation alone (Oltmans et al. 2000; Kley et al.	

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				2000)." Also the following text in paragraph 4 is to some extent conjecture: "The sizes and numbers of ice crystals....to what degree is uncertain." [Richard Allan]	
3-1205	A	36:2	36:2	Besides model parameterizations, model dynamical variability as well as uncertainties in the observed temperatures and profile and magnitude of ozone chages are also issues. [V. Ramaswamy]	
3-1206	A	36:3	36:6	In contrast to what is stated here, two papers illustrate a reasonable agreement between model simulations and observed temperature trends over the depth of the stratosphere without the need for sizeable water vapor changes. The papers are Ramaswamy and Schwarzkopf [GRL, 29, 2064, doi:10.1029/2002GL015141, 2002] and Schwarzkopf and Ramaswamy [GRL,29, 2184,doi:10.1029/2002GL015759, 2002]. The comparison between model simulations and observations has to take into account the uncertainties in the ozone profile especially near tropopause, uncertainties in temperature trend estimates, and model dynamical variability. [V. Ramaswamy]	
3-1207	A	36:3	36:6	The 2 GFDL papers considered the joint effect of well-mixed gases and ozone and demonstrated reasonable agreements for the: global-annual-mean vertical profile in stratosphere;zonal-annual-mean in the lower stratosphere (50-100 mb); and lower strat. zonal-winter and zonal-summer mean temperature trends. The GCM simulations performed differed from the "linear" sum (i.e., adding the responses due to well-mixed gases, ozone and water vapor considered separately) performed in Shine et al. [2003] when they gaged results from the different models, and differed from Langematz et al. [2003] who did not consider the non-CO2 well-mixed gases. [V. Ramaswamy]	
3-1208	A	36:6	36:6	Shine et al. (2003) [missing "("] [Richard Allan]	
3-1209	A	36:6	36:9	This conclusion is on the mark, but in the light of the satellite-balloon discrepancies, it does look like water vapor is likely not a significant reason for model failures (if at all serious ones) to accurately reproduce observations. This is where the satellite data is really packing in a punch as it reveals gaps in our inability to reconcile the observations. [V. Ramaswamy]	
3-1210	A	36:6	36:6	Before "2003" bracket missing. [Christian-D. Schoenwiese]	
3-1211	A	36:6	36:6	Shine et al (2003)) [Bernard Seguin]	
3-1212	A	36:6	36:6	Shine et al., 2003) --> Shine et al.(2003) [Koji Yamazaki]	

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3-1213	A	36:11	36:32	These paragraphs are about explaining rather than documenting changes. Could be deleted to save space. [Neville Nicholls]	
3-1214	A	36:15		Not sure I understand this. I'm no expert, but I believe most methane is oxidized near the stratopause. So if an air parcel has descended from the stratopause to the middle stratosphere, wouldn't it have most of its methane, not half of it, oxidized? [Adrian Simmons]	
3-1215	A	36:25	36:26	The statement regarding aviation water vapor emissions appears inconsistent with Chapter 2, page 22, lines 39-40, which states that aviation water emissions have an insignificant RF. As indicated in comments above the characterization of the impact of aviation water emissions needs to be reconciled throughout the report. [Lourdes Maurice]	
3-1217	A	36:34	36:50	This is too detailed - delete or reduce substantially [Neville Nicholls]	
3-1218	A	36:39	36:40	This is not a new result. It has been known for a couple of decades that deeply penetrating convective clouds, especially in the SW North Pacific, produce excess vertical velocity that inject dryer air than that expected by convection-induced dessication at the "traditional tropical tropopopause". [Jerry Mahlman]	
3-1219	A	36:46	36:48	Prior to cited references, Hatsushika and Yamazaki (2003) noted the same content as cited references using trajectory analysis of AGCM simulation. I suggest to add Hatsushika and Yamazaki(2003) into the citation. Hatsushika, H., and K. Yamazaki, 2003: The stratospheric drain over Indonesia and dehydration within the tropical tropopause layer diagnosed by air parcel trajectories. J. Geophys. Res., 108(D19), 4610, doi:10.1029/2002JD002986. [Koji Yamazaki]	
3-1220	A	37:5	37:6	50% is more than apparent! [Michael Hobbins]	
3-1222	A	37:8	37:9	This is a fair and honest statement. [Jerry Mahlman]	
3-1223	A	37:11	38:15	Due to different forcing effects it would be importabt to discern between water and ice clouds (only the latter type amplifies the greenhouse effect) [Christian-D. Schoenwiese]	
3-1224	A	37:11	39:28	This section should begin with a paragraph emphasizing the lack of consistency of the different data sets. [Marcia Baker]	

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3-1225	A	37:11	39:32	I tried to understand the conclusions of the section on clouds but found it rather difficult. Please consider ways to make this section clearer, and at a minimum add a few sentences at the end that summarize the conclusions. [Susan Solomon]	
3-1226	A	37:11	40:24	While this section discusses observed increases in cloud cover over many land areas of the world, it never relates them back to the idea that increased temperatures will lead to increased drying. It would seem that direct solar radiation is far more efficient at evaporating surface moisture than simply raising the temperature under cloudy skies. Thus, it seems that the observed increase in cloudiness would offset, to some degree, the drying that takes place. This issue needs to be addressed. [Jeffrey Kueter]	
3-1227	A	37:13	37:15	It could be stated here is that the problem with surface observations is that if there is a deck of low cloud, nothing is known about possible layers of cloud above. The converse is the case for satellite observations. [Adrian Simmons]	
3-1228	A	37:16		Change "were" to "have been". [Adrian Simmons]	
3-1229	A	37:30	37:34	Please, add the following result: Also in Italy, over the last 50 years, a highly significant negative trend in cloud cover has been observed (Maugeri et al., 2001). REFERENCE: M. Maugeri, Z. Bagnati, M. Brunetti, T. Nanni. 2001. Trends in Italian total cloud amount, 1951-1996. Geophys. Res. Let., 28, 4551-4554. [Michele BRUNETTI]	
3-1230	A	37:30	37:34	I would suggest to highlight that also in Italy a highly significant negative trend in cloud cover has been observed over the last 50 years (Maugeri et al., 2001). REFERENCE: Maugeri, M., Bagnati, Z., Brunetti, M., Nanni, T., 2001: Trends in Italian total cloud amount, 1951-1996, Geophys. Res. Lett., 28, 4551-4554. [Teresa NANNI]	
3-1231	A	37:30	37:34	The mid-level stratiform clouds over southwestern China in March has been increasing in recent decade, leading to a moderate cooling over there. See: Li Jian, Rucong Yu, Tianjun Zhou, and Bin Wang, 2005, Early Spring Cooling Trend Downstream of the Tibetan Plateau, Journal of Climate, in press [Rucong Yu]	
3-1232	A	37:30	37:34	The mid-level stratiform clouds over southwestern China in March has been increasing in recent decade, leading to a moderate cooling over there. For reference, see: Li Jian, Rucong Yu, Tianjun Zhou, and Bin Wang, 2005, Early Spring Cooling Trend Downstream of the Tibetan Plateau, Journal of Climate, in press	

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				[Tianjun ZHOU]	
3-1233	A	37:31	37:31	please insert the following sentence after "…(Kaiser, 1998)." : In Central Europe (Auer et al., 2005b) a new dataset of homogenised series of cloudness and sunshine duration starting in the 19th century shows decreasing/increasing cloudiness/sunshine trends for the entire 20th century and also for the recent 50 and 25 years. [Reinhard Böhm]	
3-1234	A	37:36	:45	Why is the SH correlation between precip anomaly and cloud cover anomaly so much larger than the NH correlation? Please explain. [Fons Baede]	
3-1235	A	37:43	37:45	Please change the last sentence to: "However, independent human observations from military stations suggest an increasing trend (~1.4% of sky per decade) in U.S. total cloud cover." Note that the USHCN DTR data did not show accompanying DTR decreases, which may be due to the fact that the U.S. total cloud cover increase comes mainly from mid and high clouds that have small effects on DTR. [Aiguo Dai]	
3-1236	A	37:57		Delete comma [Neville Nicholls]	
3-1237	A	38:0		Section 3.4.3.2 would benefit from a more logical structure, for example beginning with the consistency between products and then noting the concerning discrepancies. I suggest the following: (1) move 3.4.3.2 para 2 to line 21, after "…begin in June 1983." (2) move lines 21-34 to a separate paragraph before line 49. (3) add a summary, e.g.: "While there is some consistency between ISCCP, ERBS, SAGE II and surface observations for a reduction in high cloud cover during the 1990s (e.g. Wielicki et al. 2002a; Wang et al. 2002b; Zhang et al. 2004c; Norris 2005) there are substantial uncertainties for decadal trends in all satellite datasets (Vonder Haar and Campbell 2005; Wyle et al. 2005;Trenberth 2002) and at present there is no clear concensus on changes in cloudiness over decadal timescales." [Richard Allan]	
3-1238	A	38:10	38:10	Move (Yu et al. 2004) reference to the end of the sentence. [Richard Allan]	
3-1239	A	38:10	38:15	This section was hard to understand. Be more explicit about the directions in which cloud amount and surface temperature change. Thus I did not fully understand "in tandem". [Chris Folland]	
3-1240	A	38:10	38:15	these lines belong to 3.4.2.4 about stratospheric water vapour, but they rather concern clouds? [Bernard Seguin]	

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3-1241	A	38:10	38:15	The increased amount of stratus clouds in recent decade cools the surface air downstream of the Tibetan Plateau. see: Li Jian, Rucong Yu, Tianjun Zhou, and Bin Wang, 2005, Early Spring Cooling Trend Downstream of the Tibetan Plateau, Journal of Climate, in press [Rucong Yu]	
3-1242	A	38:10	38:15	The increased amount of stratus clouds in recent decade induces a negative net cloud radiative forcing, thereby cools the surface air downstream of the Tibetan Plateau and triggers a positive cloud-temperature feedback. For reference, see: Li Jian, Rucong Yu, Tianjun Zhou, and Bin Wang, 2005, Early Spring Cooling Trend Downstream of the Tibetan Plateau, Journal of Climate, in press [Tianjun ZHOU]	
3-1243	A	38:17	39:27	Section 3.4.3.2 could be somewhat shortened as few results clearly emerge. [Chris Folland]	
3-1244	A	38:17		The first paragraph of this section ends with a sentence suggesting ISCCP data are not good for trends. The second paragraph reports trends based on ISCCP, and the third paragraph then re-expresses doubt. Please assess whether the data are credible and if so, report trends, if not, don't. [Dian Seidel]	
3-1245	A	38:17		The most fundamental issue in comparing surface observer and satellite cloud fraction statistics is handling optically thin cloud. Recent GLAS global lidar observations suggest that cloud with optical depth less than 0.3 can make up ~ 20% of global cloud cover: yet their radiative impact will be dramatically smaller than other clouds. Note that the pdf of cloud optical depth peaks at zero optical depth: vanishingly thin cloud. The radiative impact of changing this thin cloud cover from 20% of the globe to 15% of the globe would look like a VERY large change in cloud fraction for climate change, but radiative impact would be very small: 0.15 mean optical depth is similar to aerosol SW forcing: roughly 5 W/m^2 when present, and therefore 0.05*5 = 0.25 W/m^2 in global average reflected SW flux for a 5% change in global cloud fraction! LW or OLR changes are similarily small even for thin cirrus:: visible optical depth of 0.15 = emissivity of ~ 0.07 = LW flux change of ~ 0.07*60*2 = 8.4 W/m^2 cloud radiative forcing when present, times 0.05 of the globe covered + 0.4 W/m^2 OLR change. This assumes 60K difference in cloud vs surface temperature is used as typical of thin cirrus. Note that for satellite data sets, optical depth detection threshold varies from 0.1 to 0.3 in optical depth depending on solar zenith, viewing zenith, cloud height, and solar/thermal spectral channels used. But the radiative impact of these very different total cloud fractions are small. [Bruce Wielicki]	
3-1247	A	38:26	38:26	Delete the reference to DTR as explained above.	

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				[Aiguo Dai]	
3-1248	A	38:36	38:41	Is there a punchline here? [Jerry Mahlman]	
3-1249	A	38:36	:37	This seems in disagreement with the upper panel of fig 3.4.6 [Fons Baede]	
3-1250	A	38:37		Reference to Rossow and Duenas here refers to a ~5% change, whereas the reference on p42 line 44 refers to 3-4%. Statements should be made consistent. [Adrian Simmons]	
3-1251	A	38:41	38:43	Analysis of SAGE II data also suggests a decline in cloud frequency above 12 km between 1985 and 1998 (Wang et al., 2002b), which appears to be consistent with the decrease in upper-level cloud cover noted in ISCCP and ocean surface observations." should be "Analysis of SAGE II data also reveal a decline in cloud frequency above 12 km between 1985 and 1998 (Wang et al., 2002b) that is to be consistent with the decrease in upper-level cloud cover seen in ISCCPcloud and ocean surface observations. [Patrick Minnis]	
3-1252	A	38:44	38:50	All the published versions of UAH MSU data sets must be accurately referenced here. Scientific community and governments must be informed about errors in data processing at UAH and necessity to consider invalid all previous analyses, publications and conclusions based on incorrect versions of UAH data. This information should be as objective as it is possible. [Konstantin Vinnikov]	
3-1253	A	38:46	38:47	Add reference to paper in press …… (VG2: Grody et al., 2004; Vinnikov et al., 2005), [Konstantin Vinnikov]	
3-1254	A	38:49	39:10	Discussion of the work of Chevallier et al (2005, J Climate, p2647) might be included. This paper presents time series (1979-2002) of cloud characteristics from HIRS CO2 slicing and ISCCP. [Adrian Simmons]	
3-1255	A	38:54	38:57	The independent PATMOS record is important as a confirmation of the strong diurnal variations of tropical clouds, well observed since the 1980s by geostationaries and systematized in ISCCP. The geostationaries have given a much more complete record of the diurnal cycle than could be obtaied by the 2 NOAA polar orbiters. [Robert KANDEL]	
3-1256	A	39:0		section 3.4.3.3. This section needs to refer to ch2 for more observational evidence. The two chapters are currently not consistent. This section places much too little emphasis on low clouds compared to contrails, particularly given the enormous difference in their importance (ch2).	

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				[Kenneth Carslaw]	
3-1257	A	39:1	39:14	Again, satellite cloud cover trend(?) statistics are damaged by poor data. [Jerry Mahlman]	
3-1258	A	39:4	39:4	Jacobowitz et al. found --> Jacobowitz et al.(2003) found [Koji Yamazaki]	
3-1259	A	39:12	39:22	Trends in high-level cloud cover between 1983 and 1995 from the surface and ISCCP are consistent in sign over most areas except the North Pacific, where they differ in magnitude by almost 2% per decade (Minnis et al., 2004). [Patrick Minnis]	
3-1260	A	39:17	39:17	change "artefacts" to "artifacts", remove "also" [Zhanqing Li]	
3-1261	A	39:17		after "…artifacts." Add "Note that the ISCCP total cloud amount data are a lot more reliable than the layered cloud amounts due to insufficient information discriminating cloud layers, especially for semi-transparent multi-layer clouds (Chang and Li 2005a). Often, overlapped high cirrus over low water clouds are mistakenly identified as single layer mid-level clouds by any satellite algorithms using visible and infrared data only. As a result, high and low clouds tend to be overestimated, whereas middle-level clouds are overestimated. Applying a new retrieval algorithm to the mutli-channel MODIS satellite data, Chang and Li (2005b) developed a global climatology of cloud layers showing some distinct features. A bi-mode cloud vertical structure was revealed with maximal cloud occurrence around 275 hPa and 725 hPa for high and low clouds, and an extremely low occurrence (< 4%) of mid clouds between 500-600 hPa. The global mean amounts of high, low and overlapped clouds were estimated to be 61%, 75%, 28%, respectively. The large fraction of overlapped clouds are likely to be attributed to mid-level clouds by the ISCCP or other similar products due to a lack of information content to differentiate them." [Zhanqing Li]	
3-1262	A	39:24		Although the cloud cover has not been well observed in the Arcitc, we can refer to some of the papers such as Makshtas et al. (1999) and Ikeda et al. (2003), who used human-observed cloud data collected from North Pole Stations in 1950 to 90. The trend of the cloudiness was positive all in fall, winter and spring. This trend was consistent with that in the satellite observation by Wang and Key (2003) for fall and spring, but contradictory to that in winter. The further study is strongly suggested to investigate whether low clouds are reliably observed by satellites. References1/Makshtas, A.P., E.L. Andreas, P.N. Svyashchennikov and V.F. Timachev, 1999: Accounting for clouds in sea ice models. Atmos. Res., 52, 77-113. Reference2/ Ikeda, M., J. Wang and A. Makshtas, 2003:	

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				Importance of clouds to the decaying trend and decadal variability in the Arctic ice cover. J. Meteorol. Soc. Japan, 81, 179-189. [Motoyoshi Ikeda]	
3-1263	A	39:29	39:32	These contrail radiative forcing estimates are very small, but plausible. The subject is treated more thoroughly (with a similar small global radiative forcing punchline) in the Radiative Forcing Chapter 2, First-Order Draft. [Jerry Mahlman]	
3-1264	A	39:29	40:24	Section 3.4.3.3. Most of this section may belong better in the radiative forcing or other chapters [Chris Folland]	
3-1265	A	39:31	39:34	The two statements are inconsistent. One states that aviation's contribution to cloud formation is known with great certainty, while the other states that aircraft may impact cloud formation. Need to reconcile and characterize the correlation, which is arguably stronger than "may" but not totally proven. [Lourdes Maurice]	
3-1266	A	39:41	39:43	Chapter 2 contrail forcing estimates are a little different than these and include other studies, it may be better to refer to section 2.6 in our chapter rather than quote RF numbers? [Piers Forster]	
3-1267	A	39:42	39:43	are you sure Marquadt et al did not obtain 0.0035 instead of 0.035? That is a little high considering the values reported in chap 2, pg.46, line 50 [Patrick Minnis]	
3-1268	A	39:45	39:48	Is the recent study by Ramanathan et al. (Proc. Nat. Acad. Sci., 2005) relevant here? [Nathan Gillett]	
3-1269	A	39:50	40:2	Another criticism of the Svensmark/Friis-Christensen/Marsh cloud/GCR hypothesis is how they select data-sources used in their papers. For instance in the earlier published papers (Svensmark & Friis-Christensen (1997), Svensmark (1998) and Marsh and Svensmark (2000b)) the GCR dataset used was from the Climax (Colorado, USA) cosmic ray detector. Whilst the later papers (Marsh and Svensmark (2000a;2003;2004) used the Huancayo data (Huancayo (Peru) upto 1992 and then Haleakala (Hawaii, USA)). This change is not explained in the latter papers. When the Climax GCR are correlated with low IR cloud top temperatures (CT), Marsh and Svensmark (2000b) find that the proportion of the Earth with correlations >= 0.6 is 29.6%. When the Huancayo GCR are correlated with low IR cloud top temperatures (CT), Marsh and Svensmark (2000a) find that the proportion of the Earth with correlations >= 0.6 is 34.6%, an increase of 16%. This, at the very least, suggests that the results are quite sensitive to the GCR data source chosen, something that is not considered but could be	

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				very important. This could be added here. [Gareth S. Jones]	
3-1270	A	39:50	40:24	The Marsh and Svensmark 2000 "finding", after a search based on quasi astrological preconceptions that the fault is in our Sun, was that correlation was best for clouds at low altitudes as well as low latitudes. As part of the debunking of this solar nonsense, one should cite Peter Laut's paper "Solar activity and terrestrial climate: An analysis of some purported correlations", in the J. Atm. Terr. Phys., in press in 2003. Also, in the 2002 Alpbach summer school, Friis-Christensen himself showed a slide (available on internet) showing the DEcorrelation of solar activity and global mean temperature since 1980. Finally, one should cite Damon and Laut (2004) in EOS vol. 85, no. 39, further demolishing the purported correlations based on misuse of data. [Robert KANDEL]	
3-1271	A	39:50	40:24	There is discussion of the Svensmark cloud hypothesis in chap 2 page 52 line 53 to end of section. Chapter 2 is less sceptical of it than chap 3. [Peter Stott]	
3-1272	A	39:50		This discussion of the role of cosmic rays on clouds is strongly biased and does not fairly reflect the recent work in this area. It reflects the author's prejudices rather than being a fair scientific summary. Some major flaws include: (1) There is some substantial twisting here in what is meant by the "cosmic ray-cloud hypothesis". As I understand it, the idea comes from long-standing work in ion-aerosol physics showing that cosmic ray ionisation can produce ultrafine particles and modify cloud physics. This produces the hypothesis that cosmic rays may provide a route for solar changes to influence cloud on many different timescales. It dates back to Ney (1959) in Nature. Key recent (last decade) ideas have come from Tinsley and Svensmark. Whether or not ISCCP satellite data can be used to investigate it over a decade or two is something quite different and there is considerable argument about. These two points need to be kept very separate! The hypothesis itself could also be investigated on long timescales, e.g the recent work of N.J. Shaviv, On climate response to changes in the cosmic ray flux and radiative budget, J. Geophys. Res., Vol. 110, No. A8, A08105, 10.1029/2004JA010866. Hence line 4 on page 40 is wrong...it is merely the ISCCP test of the hypothesis that has been criticized. (2) Important references are missing. Recent work on the physics comes from Yu FQ and RP Turco, 2001. From molecular clusters to nanoparticles: Role of ambient ionization in tropospheric aerosol formation, J. Geophys. Res. 106 (D5): 4797-	

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				4814. These ideas have received a major boost with the recent discovery of large cosmogenic ions in the upper troposphere: Eichkorn, S., Wilhelm, S., Aufmhoff, H., Wohlfrom, K.H., Arnold, F. 2002 Cosmic ray-induced aerosol-formation: First observational evidence from aircraft-based ion mass spectrometer measurements in the upper troposphere. Geophys Res Lett 29, 14, art no 1698. New evidence for a small yet significant cosmic ray effect on clouds based on long historical ground measurements of diffuse solar radiation measurements can be found here: R.G. Harrison and D.B. Stephenson, 2005: Empirical evidence for a non-linear effect of galactic cosmic rays on clouds,Proceedings of the Royal Society of London. Series A, submitted. The whole topic was reviewed extensively by Harrison R.G. and Carslaw K.S. Ion-aerosol-cloud processes in the lower atmosphere Reviews of Geophysics 41 (3), 1012, 10.1029/2002RG000114 (2003) Carslaw K.S., Harrison R.G. and Kirkby J. Cosmic rays, clouds and climate Science 298, 5599, (Nov 29), 1732-1737 (2002) which are key references. (3) Sun and Bradley 2002 also found a high correlation between cosmic rays and clouds in the Atlantic 1983-1991, and said "We have no explanation for this large scale correlation..." (4) There is no need to presume the timescale (page 40 line 18)...the references omitted to Yu and Turco and Harrison and Carslaw discuss the timescales! (5) Visual cloud reports (page 40 line 22) are very coarse and subjective measures, for effects which Svensmark and Marsh report as occurring at the few percent level. (6) No mention is made of the paleo evidence for a relationship between cosmic rays and climate, e.g. Bond, G.C. et al. 2001 Persistent solar influence on North Atlantic climate during the Holocene, Science 294, 2130–2136. Neff, U. et al. 2001 Strong coincidence between solar variability and the monsoon in Oman between 9 and 6 kyr ago, Nature 411, 290–293 in which the cosmogenic isotopes are used to infer solar changes, but of course more directly record cosmic ray changes. (7) page 40 line 1. Svensmark and Marsh have proposed a microphysical mechanism. It has not yet been experimentally established in the atmosphere. (8) A latitude effect has been demonstrated in the cosmic ray-cloud	

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				correlation. See the discussion in this recent paper: I.G. Usoskin, N. Marsh, G.A. Kovaltsov, K. Mursula, O.G. Gladysheva Latitudinal dependence of low cloud amount on cosmic ray induced ionization GEOPHYSICAL RESEARCH LETTERS, DOI:10.1029/2004GL019507, 2003 (9) No mention is made of the electrofreezing hypothesis of Tinsley. This is yet more evidence that this section lacks objective balance! [David Stephenson]	
3-1273	A	39:51	40:24	The cosmic ray hypothesis receives too much attention here; a statement that it is not supported by observations or by physical considerations could be enough. [Marcia Baker]	
3-1274	A	40:1	40:2	"While lacking a specific mechanism...". This section mentions the criticism of the cosmic ray-cloud hypothesis but not the microphysical explanations. There is now a very large literature providing microphysical explanations. Ch 2 (53.4-25) does a much better job in this regard so you could refer to it. [Kenneth Carslaw]	
3-1275	A	40:1	40:1	I suggest to say 'these authors' rather than enumerating their names [Bernard Seguin]	
3-1276	A	40:4	40:4	It would help here to clarify that these are "observationally based" criticisms, and not criticisms of the plausible microphysical links (see, e.g., Carslaw, Harrison and Kirkby review in Science in 2002. [Kenneth Carslaw]	
3-1277	A	40:10	40:10	This sentence does not do justice to the Marsh and Svensmark work pointing out a possible calibration problem. Simple bald statements like this are not helpful. What is your assessment of their "assertion". [Kenneth Carslaw]	
3-1278	A	40:12	40:12	Furthermore, there is no evidence for systmatic long-term trends in the cosmic ray flux or other solar or geomganetic proxies such as the 10.7 cm flux or the interplanetary magnetic field (IMF), aa-index since 1952 (Richardson et al., 2002, JGR, vol 107., doi: 10.1029/2001JA000507; Benestad 2005, GRL, 32 L15714, doi:10.1029/2005GL023621). A decrease in cloud cover due to cosmic ray flux is furthermore inconsistent with negative trends in DTR (a warming due to reduced planetary albedo is presumably only on the dayside and reduced cloud cover is likely to increase the night side OLR) as well as negative temperature trend in the stratosphere (a decrease in cosmic ray flux is purported to be due to increased solar activity which again would act to warm the stratodphere – UV warming). [Rasmus E. Benestad]	

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3-1279	A	40:12	40:12	There is no long-term trend in the cosmic ray time series (Richardson et al. (2002), JGR, vol 107, doi: 10.1029/2001JA000507; Benestad (2005) GRL, vol 32, doi:10.1029/2005GL023621). Furthermore, a decrease in GCR cannot explain a reduction in DTR (and cloudiness has apparently not decreased according to the accounts in AR4). Benestad (2005) furthermore raises some issues regarding the quality of the sunspot record before 1900. [Rasmus E. Benestad]	
3-1280	A	40:14	40:24	A reference to the paper Kristjánsson et al. (2004) would be useful here. This paper updated the analysis of Kristjánsson et al. (2002) by adding another 2 years of ISCCP data. The results mainly support the conclusions of the 2002 paper, for instance a slightly negative (not significant) correlation is found between high pass filtered cosmic ray and low cloud cover data. The full reference of the paper is: Kristjánsson, J. E., J. Kristiansen, and E. Kaas, 2004: Solar activity, cosmic rays, clouds and climate - an update. Adv. Space Res., 34, 407-415. [Jón Egill Kristjánsson]	
3-1281	A	40:14	40:24	This paragraph appears to be physically unsupportable, on a number of quantitative grounds, with no known physically supportable arguments. [Jerry Mahlman]	
3-1282	A	40:24		Clouds are such an important topic that I would have appreciated a summary statement or paragraph on cloud changes at the end of 3.4.3. [Harry Bryden]	
3-1283	A	40:24		is this section missing a summary? [Gabriele Hegerl]	
3-1284	A	40:25	40:25	Please include a summary statement on cloud changes. It would really help. [FILIPPO GIORGI]	
3-1285	A	40:26	42:10	Section 3.4.4.1. Another section that could usefully be shortened with few clear messages. [Chris Folland]	
3-1286	A	40:26		Is it possible to say anything about observations of radiative forcing due to indirect or semi-direct aerosol effects? [FILIPPO GIORGI]	
3-1287	A	40:28	42:10	The results of analyses of scanner data should not be neglected, even if there is no data series as long as that of ERBS WFOV. May I suggest a paper "Planetary Radiation Budgets" by myself (R. Kandel) and M. Viollier, which has just appeared (on-line) in Space Science Reviews, vol. 120, 1-26. The paper discusses annual cycles, interdecadal fluctuations and possible trends of ERB components. [Robert KANDEL]	

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3-1288	A	40:28	43:2	There are potential incosistencies between the discussion and summary of the radiation data and the changes in atmpspheric temperatures. Some discussion of the meaning of the radiation balance analysis for changes in atmospheric temperataure is advisable. [Henry Diaz]	
3-1289	A	40:47	40:48	The change in tropical hihigh cloud cover observed by SAGE-II also can explain much of the increase ougoing LW radiation over the Tropics (Wang et al. 2002b) proviidng addiitonal support for the decrease in high clouds over the Tropics. [Patrick Minnis]	
3-1290	A	40:49	40:49	Add references to: Hatzidimitriou, D., I. Vardavas, K. G. Pavlakis, N. Hatzianastassiou, C. Matsoukas and E. Drakakis, "On the decadal increase in the tropical mean outgoing longwave radiation for the period 1984-2000," Atmos. Chem. Phys., 4, 1419-1425, 2004; Hatzianastassiou, N., A. Fotiadi, C. Matsoukas, K. G. Pavlakis, E. Drakakis, D. Hatzidimitriou and I. Vardavas, "Long-term global distribution of Earth's shortwave radiation budget at the top of atmosphere," Atmos. Chem. Phys., 4, 1217-1235, 2004. [Richard Allan]	
3-1291	A	40:51	40:53	This sentence is an exact copy of the sentence on page 3-39 line 14 -15 and should not be repeated to save some space. [Martin Wild]	
3-1292	A	41:0		In Table 3.5., the row for "ERBS Edition 2" should be removed since they are incorrect. The apparent agreement between ERBS Edition 3 and ISCCP FD in Table 3.5. seems inconsistent with the discussion " These results suggest that the homogenization methods used by the ISCCP are inadequate for assessment of trends and climate change detection" (see 3-38, lines 32-34). [Qiang Fu]	
3-1293	A	41:7	41:28	One of the strengths of the original Wielicki et al. 2002a study was agreement between the ERBS non-scanner record and scanner data from ERBS, ScaRaB and CERES which also showed an increase in OLR and decrease in reflected SW. There is no mention of this agreement, presumably because the updated ERBS record shows a worse agreement with the scanner record. This should be at least commented upon. [Richard Allan]	
3-1294	A	41:19	41:19	No units in the Table 3.5 [Eugene Rozanov]	
3-1295	A	41:22	41:23	The Table shows astonishing variation in the TOA figures but no uncertainties. Uncertainties should be added and some comment made on the range of table entries. [Marcia Baker]	
3-1296	A	41:24	41:50	This set of assertions need to be cross checked with the appropriate authors in Chapter 2.	

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				I think that their differences in perspective can be reconciled to the benefit of both. [Jerry Mahlman]	
3-1297	A	41:28	41:28	It might be spurious as suggested in the text before. [Qiang Fu]	
3-1298	A	41:30	41:35	Randall et al. 2005 found a radiative imbalance of 0.85 Wm-2 (heating) which was being absorbed by the ocean. This seems consistent with the Willis dataset…how about ERBS? [Richard Allan]	
3-1299	A	41:30	41:30	Replace 'the majority' by 'most' or (better) a quantitative estimate. [Marcia Baker]	
3-1300	A	41:32	41:32	Which of the gloal chance estimates in Table 3.5 are consistent with the Willis data? [Marcia Baker]	
3-1301	A	41:37	41:42	Line 37 shoud read "solar photons reflected from parts of the Earth". The Wielicki et al 2005 paper (I am a co-author) was rather diplomatic in discussing shortcomings of the earthshine results. The Kandel & Viollier 2005 paper (note above) makes more explicit the spatial, temporal, angular, and spectral shortcomings of earthshine analysis. [Robert KANDEL]	
3-1302	A	41:37	41:42	The timeframe (2000-2003) where the discussion between Earthshine and CERES is focusing on should be explicitly stated. A reference should be given for the mentioned changes in ocean heat storage in support of the CERES data. [Martin Wild]	
3-1303	A	41:40	41:42	Need to change the 2 W/m^2 to 1 W/m^2: The Wielicki et al 2005 paper indicated that the 2 W/m^2 drop in SW flux from CERES was likely to be reduced to ~ 1 W/m^2 following completion of more accurate corrections of instrument optics contamination in orbit. This is in fact what resulted from further comparisons and analysis and is published in: Matthews, G. , K. Priestley, P. Spence, D. Cooper, and D. Walikainen, "Compensation for spectral darkening of short wave optics occurring on the Clouds and the Earth's Radiant Energy System," in Earth Observing Systems X, edited by James J. Butler, Proceedings of SPIE Vol. 5882 (SPIE, Bellingham, WA, 2005) Article 588212. [Bruce Wielicki]	
3-1304	A	41:40	41:42	A follow on note: comparisons of SeaWIFS 0.4-0.7um data to CERES SSF Rev 1 broadband reflected SW flux anomalies shows agreement in reflected global SW flux interannual variabiity to ~ 0.4 W/m^2. These are the two best calibrated measurements (SeaWIFS pitches over monthly to scan the moon as a stability target). Earthshine is a 6 W/m^2 signal that disagrees with both SeaWIFS and CERES. In addition, comparisons of CERES anomaly record to MODIS and MISR visible channel interannual changes show agreement to within 1 W/m^2 for MODIS and 0.5 W/m^2 for MISR. These	

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				differences are within the 2% or 2 W/m^2 broadband equivalent calibration stability uncertainty of MODIS and MISR. A GRL paper will be submitted shortly on these comparisons. A figure can be provided if desired. The major conclusion is that there are no discernable global trends over 2000 to 2005 in global SW reflected flux with an uncertainty of about 0.5 W/m^2 for 95% confidence. Deseasonalized global monthly anomalies reach a maximum value of about 1.5 W/m^2. Deseasonalized tropical mean (30S to 30N) SW reflected flux anomalies are much larger and range from -2 to +3 W/m^2. The tropical anomalies are strongly correlated with MODIS derived cloud fraction, and weakly correlated with MODIS aerosol optical depth anomalies. [Bruce Wielicki]	
3-1305	A	41:46	41:46	The abbreviation NRA should be explained. [Martin Wild]	
3-1306	A	42:1	42:10	This set of assertions need to be cross checked with the appropriate authors in Chapter 2. I think that their differences in perspective can be reconciled to the benefit of both. [Jerry Mahlman]	
3-1307	A	42:3	43:	The suggestion that direct absorbing aerosol forcing was the source for global scale dimming was made by Ramanathan et al (Science, 2001) who also gave the aerosol induced global scale dimming is in the range of -2 to -4 W for direct forcing and -0.5 to -1.5 W for indirect forcing. It is important to note that the dimming by absorbing aerosols is largely compensated by a correspomnding increase in atmospheric solar heating , with very little change at TOA; thus the large dimming does not pose a conflict with global warming. In another paper, Ramanathan et al (PNAS, 2005) show that the dimming of bout 7% over India since 1950 is due to absorbing aerosols. However if we follow Liepert and others and assume that the dimming was due to changes in clouds, then the -7 Wm-2 to -15 Wm-2 dimming by Liepert and others will translate into a -7 to -15 Wm-2 reduction at TOA during 1950 to 1990 which will pose a huge conflict for the whole IPCC report: why did the climate system not cool signficantly and why did the heat capacity of the ocean incraese during the middle of this huge dimming. [Veerabhadran Ramanathan]	
3-1308	A	42:6	42:10	One might add (see K&V above, p. 16) that "The annual cycle of planetary net TOA radiative flux is roughly consistent with the independently determined annual cycle of heat content of the world ocean." [Robert KANDEL]	
3-1309	A	42:6	42:10	This is a nice, honest appraisal. [Jerry Mahlman]	
3-1310	A	42:14	43:55	The text of section 3.4.4.2 and Box 3.1 is rather confusing. I did not end up with a clear enough understanding of these quite important issues.	

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				[Chris Folland]	
3-1311	A	42:14		Section 3.4.4.2 - The suggestion in this section that there existed a worldwide reduction in surface solar irradiance between 1961 and 1990 needs to be reconsidered. The total of the published work on the topic prior to 2005 were for observations from highly non-uniformly distributed land-only sites, some with questionable quality data records, and which grossly under-represent the entire surface of the planet. None of the papers, or all in concert, ever demonstrated that the suggested decrease is solar irradiance was truly worldwide or global in extent. The term "global dimming" was applied to the phenomenon by some people in reference to a published use of the term, but in that case it referred to the terminology applied to total solar irradiance (diffuse + direct) as typically measured by a pyranometer. Further contradicting the idea that these land based stations represent a worldwide phenomena is a recent paper by Alpert et al., 2005: Geophys Res. Let., 32, L17802, doi:10.1029/2005GL023320, shows that most all the downward trends were caused by dominant signals from sites near major population centers. The fact that the dimming may not have been global phenomenon also helps explain why there were apparent discrepancies and conflicts with pan-evaporation records at yet other locations and times, such as that were the subject or reference work by Roderick and Farquhar, the subject of a subsequent comment. [Ellsworth Dutton]	
3-1312	A	42:14		section 3.4.4.2. I think this is an excellent summary on the recent detection of variations in surface solar radiation. I have only few minor comments [Martin Wild]	
3-1313	A	42:18	42:21	Please remove the 2 sentences from " Measurements of … to …water vapor feed back effect.", since Philipona et al.'s measurements show also longwave radiation and the full radiation budget. [Rolf Philipona]	
3-1314	A	42:19	42:19	Philipona and Dürr 2004: missing co-authors. This should be Philipona et al (2004) and in the reference list on page 3-101 should be expanded to: Philipona, R, Dürr, B., Marty, C. Ohmura, A., and Wild, M., 2004: Radiative forcing - measured at Earth's surface -corroborate the increasing greenhouse effect, Geophys. Res. Lett., 31, L03202 10.1029/2003GL01876. [Martin Wild]	
3-1315	A	42:22	42:32	Suggest shortening to: "A reduction in downward solar radiation of 1.3% decade–1 or	

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				about 7 W m–2 was observed from 1961 to 1990 at stations worldwide (Liepert, 2002; Gilgen et al., 1998). Surface solar radiation was also found to decline globally including sites in the Arctic and Antarctic (Stanhil and Cohen, 2001) as well as at sites in the former Soviet Union (Abakumova et al., 1996; Russak, 1990), around the Mediterranean Sea (Omran, 2000 and Aksoy, 1997), the United States (Liepert, 2002), and Southern Africa (Power and Mills, 2005). On the other hand, the changes at sites in Europe were rather mixed from 1960 to 1990. Alpert et al. (2005) provide evidence that a significant component of these reductions may relate to increased urbanisation over the period. [REF: Alpert, P., P. Kishcha, Y. J. Kaufman, and R. Schwarzbard (2005), Global dimming or local dimming?: Effect of urbanization on sunlight availability, Geophys. Res. Lett., 32, L17802, doi:10.1029/2005GL023320.] [Richard Allan]	
3-1316	A	42:22	42:26	Use consistent units (% or W/m2) [Kenneth Carslaw]	
3-1317	A	42:27	42:28	Number of surface solar radiation stations declined by 60%? This seems quite dramatic if it refers to the solar radiation levels. [Rasmus E. Benestad]	
3-1318	A	42:33	42:33	the word "drops" should be replaced by something else, say "declines", otherwise "cloud drops" come to the mind [V. Ramaswamy]	
3-1319	A	42:33	42:37	Do you want to include the possibility of cloudiness being influenced by aerosols, as in the range of mechanisms outlined in Chapter 7? There is of course the difficulty of separating any "non-aerosol-related" effects in the observed cloud changes. [V. Ramaswamy]	
3-1320	A	42:33	42:37	The reduction of sunshine duration and solar radiation in North China or eastern China have also been found in some recent studies such as those by Xu, X. and Tang, X. et al., 2002, An introduction to urban environmental meteorology, China Meteorological Press, Beijing (In Chinese); Zhang Y., 2003, Analysis of solar radiation variations over Nanjing region during recent 40 years. J. of Geographical Sciences, 13(1): 97-104 (In Chinese); Ren, G. Y., Guo, J., Xu, M. Z., et al., 2005, Climate changes of Mainland China over the past half century, Acta Meteorologica Sinica, 63 (5) (in press in Chinese) [Guoyu REN]	
3-1321	A	42:39	42:53	Section is at significant odds with the cloud section that is arguing for large uncertainties in clouds, whereas here they are implied to be very much more certain. Both positions cannot be true. I may have missed other such cases in the chapter so it is important to go back and check for contradictions between all sections. These are likely to relate primarily (probably) to the implied uncertainty in different datasets / data types.	

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				[Peter Thorne]	
3-1322	A	42:39	42:39	Over Europe, in Wild et al 2005, we sampled 300 sites into 32 gridcells (not stations), of which 26 cells showed rather a brightening than a dimming. Over Japan we sampled 45 stations in a similar way. In addition we used 76 worldwide distributed sites. From these sites, the majority showed a recovery. A possible reformulation could therefore be: instead of 'At 26 out of 32 analyzed sites worldwide' : 'At a majority of numerous analyzed sites worldwide …', or more quantitatively 'At the majority of 421 analyzed sites worldwide….', [Martin Wild]	
3-1323	A	42:43	42:48	Suggest shortening to: "The increase in surface solar radiation ("brightening") agrees with satellite and surface observations of reduced cloud cover (Wielicki et al., 2002a; Wang et al. 2002b; Rossow and Dueñas, 2004; Pinker et al., 2005; Norris 2005) although thre is evidence that some of these changes are spurious (Section 3.4.3)." [Richard Allan]	
3-1324	A	42:44		See comment 40. [Adrian Simmons]	
3-1325	A	42:45	42:47	Data reported by Pinker et al (2005) actually show a slight dimming over the land - the reverse of the study by Wild et al. (2005). The brightening reported by Pinker et al. is over the ocean. This difference should be clearly spelt out. [Michael Roderick]	
3-1326	A	42:49	42:50	There is no reason to expect local site changes to agree with global radiation changes. Regional changes in radiation are an order of magnitude larger than global or tropical mean. I do not understand the relevance of comparing a few local sites to global numbers: it is an apples and oranges comparison and should be eliminated. Global change is not uniform, especially for radiation and precipitation. [Bruce Wielicki]	
3-1327	A	42:50	42:50	Trepte and Winkler, 2004 are the wrong reference here: their paper deals with UV-radiation (erythemal UV-doses to be more precise) and not with total solar irradiance and they do not report "a continued decline in solar radiation" but an increase of erithemal UV-doses at clear days and a decrease of cloudiness in summer months. [Reinhard Böhm]	
3-1328	A	42:50	42:50	The continued decline in solar radiation at remote sites in China in the last decade is consistent with the strong increase in pollution (Richter et al., 2005). REFERENCE:	

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				Richter, A., Burrows, J.P., Nüss, H., Granier, C., Niemeier, U., 2005, Increase in tropospheric nitrogen dioxide over China observed from space, Nature, Volume 437, Issue 7055, 1 September 2005, Pages 129-132. [Michele BRUNETTI]	
3-1329	A	42:50	42:53	The point about puzzling declines in pan ET was mentioned previously with reference to the Box 3.1 already. Perhaps it should be in one place only (e.g. under the evaporation section). [Richard Fernandes]	
3-1331	A	42:50	42:50	Philipona and Dürr 2004: missing co-authors. This should be Philipona et al (2004) and in the reference list on page 3-101 should be expanded to: Philipona, R, Dürr, B., Marty, C. Ohmura, A., and Wild, M., 2004: Radiative forcing - measured at Earth's surface -corroborate the increasing greenhouse effect, Geophys. Res. Lett., 31, L03202 10.1029/2003GL01876. [Martin Wild]	
3-1332	A	42:50	42:50	The Chinese data also do not show a decline anymore during the 1990s, but rather leveling off or slight recovery (Liu et al., JGR 2004, their Fig.6, Wild et al, Science 2005, supporting online material fig S9), and are therefore in line with the general tendency of a recovery. [Martin Wild]	
3-1333	A	42:50	43:52	Why puzzling? Their trend analyses were longer-term than just the 1990's, going back to 1970 and 1975. [Michael Hobbins]	
3-1335	A	42:51	42:53	The paper by Roderick & Farquhar (2004) did not interpret the decline in pan evaporation as ongoing dimming. They said that this was one possible reason and gave others as well (e.g. declining wind). A more recent paper by those authors (Roderick & Farquhar, 2005, Int. J Climatology, in press) has also found an ongoing decline in pan evaporation in New Zealand. This latter paper will be printed in the December edition of the journal and can be cited by the IPCC. [Michael Roderick]	
3-1336	A	42:54	42:54	There are three GRL papers published on the temperature increase and radiation change in Europe by Philipona and co-workers (a last one in October 2005). I would therefore like to suggest that a separate paragraph is made after paragraph 2 of this subchapter. Please add the paragraph below: [Rolf Philipona]	

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3-1337	A	42:54	42:54	Measurements of longwave radiation are generally only available since the 1990s. With eight stations of the Alpine Surface Radiation Budget network in Central Europe, Philipona and Dürr (2004) show solar shortwave radiation decreasing from 1981 to 2002. Radiation budget measurements from 1995 to 2002 show shortwave radiation also decreasing, while longwave downward radiation strongly increases (Philipona et al., 2004). The total surface absorbed radiation, net shortwave plus longwave downward, shows high correlation with the rapid temperature increase. Very high correlation however, is found between longwave downward radiation and strongly increasing temperature and specific humidity under cloud-free situations. In an extensive study contrasting individual radiation fluxes with surface temperature and specific humidity as well as temperature and integrated water vapor evolution over large parts of Europe, Philipona et al., (2005) show, that neither solar radiation including aerosol effects, nor clouds play an important role on the surface temperature increase. Instead, their measurements show rapid greenhouse warming and manifest strong water vapor feedback that make up for 70% of the longwave radiation increase and hence the warming in Central and Northeastern Europe. [Rolf Philipona]	
3-1338	A	42:55	43:2	I didn't understand this paragraph. Surely a decrease in surface radiative heating implies a decrease in temperatures? [Nathan Gillett]	
3-1339	A	43:0		Box 3.1. Dimming of the Planet. I am not confident that I can supply any valuable analysis to the "Dimming Problem", because I have not studied it in any quantitative or physical detail. [Jerry Mahlman]	
3-1340	A	43:1		It is not at all obvious that the residence time of water vapour in the atmosphere changes significantly in a warmer, wetter atmosphere. Is there a physical argument behind this assertion? It is straightforwardly calculatable in any good climate model. I am guessing that that residence time won't change importantly from the model's control run. [Jerry Mahlman]	
3-1341	A	43:6		Box 3.1. The discussion on trends in radiation and the additional information in this box is generally very well handled. However, I was confused by some aspscts of the box. It seems to deal with Pan evap versus actual evap rather than Pan evap versus radiation changes as I expected from the main text. [Kenneth Carslaw]	
3-1342	A	43:6		Box 3.1 - The premise of this box is that there was a reduction in surface solar irradiance over the entire planet, which can discounted as an established fact as discussed in this reviewer's previous comment. Given that there was no well established evidence for a	

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				global or worldwide reduction in surface solar irradiance, then there is no apparent conflict or paradox with conflicting results from pan-evaporation, again at sites that dramatically under-represent the entire planet and in most cases are at sites where no "dimming" was observed in actual solar measurements. [Ellsworth Dutton]	
3-1343	A	43:9	43:9	It is not global dimming, but rather "solar dimming." As I understand it, this has happened mainly in polluted regions of the Northern Hemisphere, but is not global. [Alan Robock]	
3-1344	A	43:9	43:38	The framework proposed by Brutsaert & Parlange (1998) is based on the complementary theory and can be applied in water-limited environments. Their theory say that - as the surface warms, we expect increasing (actual) evaporation and in water-limited environments, this is usually associated with decreasing potential evaporation. Hence, the decline in pan evaporation in arid conditions implies more actual evaporation. This of course implies more rainfall, but that has happened as well. This is of course the reverse of the dogma in this chapter (and in others, e.g. Chapter 11) that warming enhances potential evaporation. Also see comment 15 for an even more general approach. [Michael Roderick]	
3-1345	A	43:9	43:14	As recently pointed out by Ramirez et al. (2005, Geophysical Research Letters, 32, L15401), the Brutsaert & Parlange (1998) interpretation is not a replacement of the Budyko-type interpretation. Rather, it is an extra principle. The trends in pan evaporation can be easily interpreted using the more general Budyko framework. See Roderick & Farquhar (2004, Int J Climatology 24, 1077-1090). This says that in water-limited environments, evaporation is less than potential evaporation. Hence, in a water-limited environment, changes in evaporation are dominated by changes in rainfall, i.e. more rainfall usually means more evaporation. At the other extreme, in energy-limited (i,e. water abundant) environment, evaporation is limited by available energy so that declining pan (and potential) evaporation also means declining evaporation. This has been observed in the former Soviet Union, where pan evaporation has decreased and actual evaporation increased in water-limited places while actual and pan evaporation have both decreased in energy-limited places (Golubev et al. 2001). Also see comment 16 where this research is cited incorrectly. [Michael Roderick]	
3-1346	A	43:10	43:10	Replace "till" by "until". [Martin Stendel]	
3-1348	A	43:11	43:12	It would be helpful if the sign of the trends in pan evaporation and actual evaporation are	

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				stated here in the first paragraph. [Nathan Gillett]	
3-1349	A	43:11	43:14	Here "considerable confusion in the literature over conflicting trends in pan evaporation and actual evaporation" is stated. In fact, the papers cited here are not confused: between them they present a coherent picture that resolves the pan evaporation paradox--explaining why, for example, Epan decreases while Tair increses or ETa increases--but that is in conflict with the outdated paradigm permeating this chapter. [Michael Hobbins]	
3-1351	A	43:12	43:12	That the trends are opposite does not mean that they are "conflicting" [Michael Hobbins]	
3-1352	A	43:12	43:12	Ref. comment #2 above: "evaporation" should read "evapotranspiration" [Michael Hobbins]	
3-1355	A	43:16		Mention of evapotranspiration here and elsewhere in this box is perhaps best omitted, or expanded to include reference to the control of transpiration by plants when root-zone soil moisture is scarce and temperatures high. No mention also of effect of windspeed (or changes in windspeed) on evaporation. [Adrian Simmons]	
3-1356	A	43:18	43:19	Pan evaporation are not measuring the potential evaporation: they are only an indicator of it, because of ttheir limited size [Bernard Seguin]	
3-1357	A	43:19	43:19	Pans measure the potential evaporation that would take place if the surface were wet at a point, which is a different measure than if the surroundings were also wet. Again, this underlines the confusion in the minds of the writers. [Michael Hobbins]	
3-1359	A	43:22		Period goes inside the right parenthesis [Ian Simmonds]	
3-1360	A	43:23	43:23	Ref. comment #2 above: "evaporation" should read "actual evapotranspiration" [Michael Hobbins]	
3-1361	A	43:23	43:28	Here the pan evaporation paradox is resolved in favor of a large-scale argument about circulation patterns, cloudiness, and precipitation that is simply unnecessary: the complementary relationship resolves the paradox (Hobbins et al., 2004; Ramirez, J. A., M. T. Hobbins, and T. C. Brown, 2005: Observational evidence of the complementary relationship in regional evapotranspiration lends streong support for Bouchet's hypothesis. Geophysical Research Letters, 32, L15401). The argument here underscores this chapter's continual disconnect between Epan, ETp, and solar radiation: the decrease in solar radiation that they say is decreasing ETa seems to have no effect (according to this	

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				chapter) on Epan. This is not a resolution of the paradox. [Michael Hobbins]	
3-1364	A	43:25	43:25	Ref. comment #2 above: "evaporation" should read "actual evapotranspiration" [Michael Hobbins]	
3-1366	A	43:26	43:27	Elsewhere in the chapter the impression is given that the trends in cloud and precipitation are very uncertain, perhaps even in sign. [Nathan Gillett]	
3-1367	A	43:26	43:26	Ref. comment #2 above: "evaporation" should read "evapotranspiration" [Michael Hobbins]	
3-1369	A	43:26	43:32	Minnis et al. (2004) determined that total cloud cover decreased over most land areas except the US, on average, between 1971 and 1995. This is at odds with the statements in these lines about cloud cover oincreasing over most land areas. [Patrick Minnis]	
3-1370	A	43:26	43:38	That is not true for most part of China or east Asia. For this region, not every sites experienced an increased precipitation and clouds. Long time and serious drought mainly induced by declined rainfall has been recorded for most parts of temperate east Asia (35-50 N) (Ren, G. Y., Guo, J., Xu, M. Z., et al., 2005, Climate changes of Mainland China over the past half century, Acta Meteorologica Sinica, 63 (5) (in press in Chinese)). Current evidence from North China does not support the compensatory relationship between pan evaporation and actual evaporation in the past 50 years (Guo and Ren, 2005, Recent change of pan evaporation and the possible climate factors over the Huang-Huai-Hai Watersheds, China, Advances in Water Science, 16 (5), 666-672 (in Chinese); Chen, D. Gao, G., Xu, C. et al. 2005, Comparison of the Thornthwaite method and pan data with the standard Penman-Monteith estimates of reference evapotranspiration in China, Climate Research, 28, 123-132) [Guoyu REN]	
3-1371	A	43:31	43:31	Ref. comment #2 above: "evaporation" should read "evapotranspiration" [Michael Hobbins]	
3-1373	A	43:32	43:38	The data reported by Golubev et al (2001) show increased evaporation in water-limited environments (because of increased rainfall) but decreased evaporation in wet regions because of decreased energy availability. This is consistent with expectations - see comment 15 - please modify the paragraph accordingly because at the moment it reads as if actual evaporation has increased everywhere. It has not. [Michael Roderick]	
3-1374	A	43:33	43:33	Ref. comment #2 above: "evaporation" should read "evapotranspiration" [Michael Hobbins]	

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3-1376	A	43:34	43:34	What type of evapotranspiration is referred to here? [Michael Hobbins]	
3-1378	A	43:37	43:37	Add: "and this may point to the decrease of the corresponding wind speed" after the word "evaporation" [Mohamed El-Shahawy]	
3-1379	A	43:38	43:38	Add the following sentence at the end of this paragraph or inside the paragraph: "Robock et al. (2005), attributed the upward trend in observed soil moisture in the Ukraine for the period 1958 to about 1980 to decreased evapotranspiration caused by solar dimming, as the upward trend could not be explained solely by an upward trend in precipitation." ref: Robock, Alan, Mingquan Mu, Konstantin Vinnikov, Iryna V. Trofimova, and Tatyjana I. Adamenko, 2005: Forty five years of observed soil moisture in the Ukraine: No summer desiccation (yet). Geophys. Res. Lett., 32, L03401, doi:10.1029/2004GL021914. [Alan Robock]	
3-1380	A	43:46	43:55	Over the period 1960-1990 considered in the paper of Wild et al (2004) under discussion in this section, land precipitation may not have increased as assumed in the criticism raised against this paper. It is shown in the report, e.g. on page 3.18 Table 3.4 and also in Figure 3.3.1 that land precipitation in the relevant decades (1960 -1990) rather decreased than increased. This is found in most datasets, and the only increasing trend in Table 3.4 for the 1951-2004 period (GPCC) stems mainly from the significant increase in the 1990s (Juergen Grieser personal communication) not covered in Wild et al. 2005. Also mentioned in Chapter 3 (page 3-22 line 9) is that global land evaporation closely follows variations in land precipitation (Quian et al 2005). The decrease in evaporation proposed by Wild et al (1990) for the solar dimming period 1960-1990 is therefore not in contradiction with major findings in AR4, and there is no clear evidence why the bowen ratio should shift in favour of a higher evaporation and less sensible heat under decreasing tendencies of precipitation and surface radiative heating. Also, the increased soil moisture observed in selected regions may well be a result of decreased evaporation as noted in Robock et al. 2005 GRL p3. An increase of land precipitation over past decades which is inconsistent with table 3.4 and figure 3.3.1 is also used in the argumentation line in the above paragraph lines 23-38 and may needs to be revised to be consistent with the findings in Chapter 3 on decreased land precipitation over the period in discussion. [Martin Wild]	

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3-1381	A	43:47	43:47	Ref. comment #2 above: "evaporation" should read "actual evapotranspiration" [Michael Hobbins]	
3-1383	A	43:48	43:49	"Of course… opposite direction." I am not sure what this sentence means: oposite direction to what? The increasing Tair, the decreasing ETa, the decreasing solar radiation? [Michael Hobbins]	
3-1385	A	43:51	43:51	Ref. comment #2 above: "evaporation" should read "actual evapotranspiration" [Michael Hobbins]	
3-1387	A	43:51	43:51	heating is not appropriate: energy woul be more appropriate (because in fact evaporation is a cooling process) [Bernard Seguin]	
3-1388	A	44:0	50:	Section 3.5: It would be more informative to remove all the summary paragraphs at the end of each subsection and distil this information in the actual summary section (3.5.7). Also, there seems to be little or no mention of changes in atmospheric circulation, for example in relation to clouds (Bony et al. 2004; Chen et al. 2002). [Richard Allan]	
3-1389	A	44:1	44:22	This paragraph is quite important, but its value will be clear only to a small set of specialists(I am one of them). Thus, it is unlikely to be of tutorial value concerning regional-scale detection/attribution of observed regional "trends". [Jerry Mahlman]	
3-1390	A	44:2	50:31	In 3.5 many acronyms such as NRA, ERA, NAM, PNA are used with no proper explanation. Although a few of them are defined in Box 3.3, it is located too far to give ideas on the acronyms at the right time. [kyung-ryul Kim]	
3-1391	A	44:2		The changes in atmospheric circulation are often derived from the reanalyses data. Does the quality of these data have similar impact on the circulation changes and temperature trends? [Qiang Fu]	
3-1392	A	44:4	44:4	First sentence: needs clarifying to say that many of the causes of atmospheric circulation variability may be natural. Natural variability may regionally enhance or act against global warming and may itself also be directly influenced by anthropogenic forcing. Where regional natural variability (perhaps temporarily) enhances the effects of global warming, the combined effect may cause markedly earlier regional impacts on climate than expected from global warming alone, and vice versa [Chris Folland]	
3-1393	A	44:17	44:17	Actually, section 3.5 does not "assess atmospheric circulation changes since the TAR" nor should it. Rather it should assess results pertaining to circulation change published since	

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				the TAR. [Dian Seidel]	
3-1394	A	44:17		"This section assesses atmospheric circulation changes (since the TAR)". No, it does not. It assess atmospheric circulation changes over a much longer period, emphasizing what new has been learnt since the TAR. [Adrian Simmons]	
3-1395	A	44:24	44:52	Refer to new papers by Gillett (Nature, 2005) and Gillett et al (J. Climate, 2005). Consider the evidence that the westerly flow, at least in winter in NH, may be starting to decline again. [Chris Folland]	
3-1396	A	44:24	44:52	Section 3.5.1 is a clear example that authors give emphasis almost only to the results concerning the last decades. I do not completely agree with this idea. In my opinion, the chapter should also highlight the efforts performed on secular records. In this context it should be interesting to highlight some interesting activities performed on the Sea Level Pressure records such as the ones of the ADVICE project. An interesting reference concerning the methodology of managing very long Sea Level Pressure records is Maugeri et al., 2004. REFERENCE: Maugeri, M., Brunetti, M., Monti, F., Nanni, T., 2004: Sea-level pressure variability in the Po Plain (1765-2000) from homogenised daily secular records, Int. J. Climatol., 24, 437-455. [Teresa NANNI]	
3-1397	A	44:26	44:29	I cant see the purpose of this paragraph. Could delete. [Neville Nicholls]	
3-1398	A	44:33	44:36	Are the pressure trends derived from annual means of sea level pressure, or other? In both cases, it would be worth to specify. [Franco Desiato]	
3-1399	A	44:33	44:36	Gillett et al. (2003) has since been updated - trends were analysed up to 2005, and a new version of the HadSLP dataset was used. These results are described by Gillett et al. (2005, GRL, 32(19), L19714), and a plot of the observed trends is included in chapter 9 (Fig 9.5.2). Results were generally consistent with earlier findings. [Nathan Gillett]	
3-1400	A	44:34		Change "for 1948 to 1998" to "from 1948 to 1998". [Adrian Simmons]	
3-1401	A	44:35	44:36	An outstanding winter SLP increase in Southern Europe was also found for the recent CLINO period (1961-1990) by Schönwiese and Rapp (see list of references, p.104, lines 41-42).	

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				[Christian-D. Schoenwiese]	
3-1402	A	44:40	44:52	There are some lines of theoretical reasoning that lead us to expect that the "tightening" of the annular "modes" may be global warming related. Climate models suggest that, as the climate warms, the total poleward moist static energy flux stays about the same, while Cp [v't]' decreases and L[v'q'] increases(thus, decreasing the amplitude of cyclonic disturbances, and acting to zonalize the westerlies. If this argument is correct, the polar annular modes may be here to stay in our warmer, wetter climate. This concept may help to increase the rigor, and decrease the level of descriptive discussion that is presented on page 45, lines 1-41. [Jerry Mahlman]	
3-1403	A	44:41	44:43	"Wang et al. 2005a" (see Comment #12 below) also assess spurious trends in both NRA and ERA-40 in terms of cyclone activity over the SH. So, this paper should be cited right after "Trenberth and Smith, 2005". [Xiaolan L. WANG]	
3-1404	A	44:54	45:41	In this section, some of the concepts and language are starting to depart significantly from the "New Scientist" or "Scientific American" level of language that was maintained in the TAR about atmospheric circulation. Has there been a policy change in this respect for the Fourth Assessment Report? E.g. further explanation would be needed if lines 27-28 or 30-31 remain as they are. Many policymakers' advisors, or scientists with a limited knowl;edge of climate science, (the TAR audience) would struggle. I believe section 3.5 needs rewriting to bring it nearer the above ideal and very likely appreciably shorten it in the process - some of the complicated detail is not obviously very relevant to climate change policy. [Chris Folland]	
3-1405	A	44:56	45:25	Discusses the geopotential height trends coming from our GRL paper which was rejected. The information is accurate, but there will not be a published source to refer to (i.e., the Fogt and Bromwich (2005) paper will not be printed and needs to be removed from your references and in this section). [David / Ryan Bromwich / Fogt]	
3-1406	A	44:56	45:25	The trends in the NH are discussed (again referred to Fig. 3.5.1), which to our knowledge have no reference even in preparation. [David / Ryan Bromwich / Fogt]	
3-1407	A	45:1	45:27	Reference to NRA and ERA-40 should be stylistically uniform. RA stands for reanalysis in each acronym, but the text refers to "NRA", but "ERA-40 reanalyses". [Adrian Simmons]	
3-1408	A	45:2		"decrease" and "incease" should be past tense. [Neville Nicholls]	

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3-1409	A	45:7		Add "relative to its exterior" after "warming of the vortex core". The statement about consistency would not be true if the vortex core warmed but there was even more warming outside the vortex. [Adrian Simmons]	
3-1410	A	45:9	45:9	60N is a better reference for the large changes than the 30N currently suggested. [David / Ryan Bromwich / Fogt]	
3-1411	A	45:12	45:12	60N is a better reference for the large changes than the 30N currently suggested. [David / Ryan Bromwich / Fogt]	
3-1412	A	45:19	45:22	It might be clearer to indicate that the SAM has shifted into its POSITIVE phase, with decreasing (increasing) PMSL over the Antarctic (mid-latitudes). [John Turner]	
3-1413	A	45:22	45:25	Current thinking is that the summer increase in the westerlies, as the SAM has gone positive, is responsible for the increases in summer season temperatures, which are most pronounced on the eastern side of the Antarctic Peninsula. The larger winter warming on the western side of the Peninsula does not seem to be linked to changes in the annular mode, rather is related to a decrease in sea ice as the northerly winds have increased since the 1950s. [John Turner]	
3-1415	A	45:37	45:38	Insert "in all sesons" between "hemispheres" and "increases" at line 38? [Franco Desiato]	
3-1416	A	45:40		"go along with" is too colloquial - replace with "accompanied"? And re-write into past tense (since the trends appeared in the past. [Neville Nicholls]	
3-1417	A	45:45	47:21	Much of 3.8 was about "Blocking" and "Storm Tracks" so it was unclear to me whether or not the earlier sections on "Blocking" and "Storm Tracks" were redundant. I know they came first, but they seemed to fit more naturally in 3.8 [Harry Bryden]	
3-1418	A	45:47		Section 3.5.3. Should this section be completed with the data from 2005 ? [Philippe Tulkens]	
3-1419	A	45:53	45:53	Replace "Wang et al. (2005)" with "Wang et al. (2005a)" (see also Comment #12 below). [Xiaolan L. WANG]	
3-1420	A	45:53	45:53	"143 km" should be updated to "181 km". [Xiaolan L. WANG]	
3-1421	A	45:54	45:54	Suggest replace "during the past half century" with "from period 1958-1977 to period 1982-2001". [Xiaolan L. WANG]	

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3-1422	A	45:54	45:54	Add "Wang et al. (2005b)" right after "corroborated by" (see also Comment #14 below). [Xiaolan L. WANG]	
3-1423	A	46:6		Delete "In contrast to linear theories of baroclinic instability". It's not necessarily so. Linear theories relate to the growth rate of small disturbances, and such growth rates do not necessarily determine the amplitudes reached by mature disturances and hence storm-track activity. Moreover, growth rates of disturbances might in any case be as large for a weakening jet if either there is a compensatory moisture increase or there is a reduction in surface westerlies that is similar to the reduction in the upper-level jet. [Adrian Simmons]	
3-1424	A	46:9	46:11	An open question still remains: is the baroclinicity or the available potential energy changing? [Paolo Michele Ruti]	
3-1425	A	46:15	46:23	About changes in the observing system: significant differences between NCEP and ERA40 reanalysis in representing the baroclinic available energy conversion processes in the pre-satellite period have been shown in a recent paper by Dell'Aquila et al. 2005 (Dell'Aquila, A., V. Lucarini, P.M. Ruti, S. Calmanti, 2005: Hayashi spectra of the northern hemisphere mid-latitudes atmospheric variability in the NCEP-NCAR and ECMWF reanalyses. Climate Dynamics, DOI 10.1007/s00382-005-0048-x). [SUSANNA CORTI]	
3-1426	A	46:15	46:32	We may have to be careful not to over-attribute these "trends", indeed if they are even "trends" in the sense of this Chapter 3 focus. [Jerry Mahlman]	
3-1427	A	46:15	46:23	SUGGESTION FOR A SUPPLEMENTARY STATEMENT: regarding the impact of the observing system changes, Dell'Aquila et al.(2005) have found discrepancies between NCEP-NCAR and ECMWF reanalyses in representing the baroclinic available energy conversion processes in the pre-satellite period. (Dell'Aquila, A., V. Lucarini, P.M. Ruti, S. Calmanti, 2005: Hayashi spectra of the northern hemisphere mid-latitudes atmospheric variability in the NCEP-NCAR and ECMWF reanalyses. Climate Dynamics, DOI 10.1007/s00382-005-0048-x) [Paolo Michele Ruti]	
3-1428	A	46:15	46:23	This paragraph refers generally to "reanalysis data". It should be made clear whether the remarks apply only to the NRA, only to ERA-40, or to both the NRA and ERA-40. [Adrian Simmons]	
3-1429	A	46:32	46:32	Suggest add the following (or the like) to the end of the paragraph: Using Canadian station pressure data for 1953-2002, Wang et al. (2005b) found that winter cyclone activity has become more frequent/durable and stronger over the lower Canadian Arctic, but less frequent and weaker in southern Canada, that summer cyclone activity has	

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				become more frequent in the Canadian east coast, and that cyclone activity over Canada is closely related to major teleconnection patterns (especially the NAO)." [Xiaolan L. WANG]	
3-1430	A	46:36	46:38	Clarify this is winter rainfall. The following new paper is relevant: Baines, P.G. 2005 Long-term variations in winter rainfall of Southwest Australia and the African monsoon. Aust. Met. Mag., 54, 91-102. [Chris Folland]	
3-1431	A	46:38	46:40	I don't understand this sentence, or how it fits with the previous sentences. Again, use the past tense to describe past changes. [Neville Nicholls]	
3-1432	A	46:39	46:40	Replace these two lines with "NRA in the SH (greater strong-cyclone activity and less weak-cyclone activity over all oceanic areas south of 40S in all seasons, and stronger cyclone activity over most areas of the austral subtropics in the warm seasons, in ERA-40) are amplified in the early decades, especially over the austral subtropics (Wang et al., 2005a)." [Xiaolan L. WANG]	
3-1433	A	46:42	46:46	The conclucion on significant increase in NH storm track activity does not seem very solid to me based on what is presented in the section. The definition of "activity" is a bit ambiguous and perhaps a more precise definition would help (is it strength?, number?, both?). [FILIPPO GIORGI]	
3-1434	A	46:42	46:46	This statement is to vague and does not provide guidance to policy makers. Either remove the statement or characterize the magnitude of the uncertainties and their likely impacts to a decision maker. [Lourdes Maurice]	
3-1435	A	46:43	46:43	Change "exact magnitude" with "magnitude". [Aristita Busuioc]	
3-1436	A	46:50		Suggest the following re-write: "Blocking events involve split flow and a displacement of the mid-latitude westerly winds….." [Anthony Lupo]	
3-1437	A	46:52	47:4	Barriopedro et al. (2005) is accepted and in press (J. Clim.) [Anthony Lupo]	
3-1438	A	46:54	46:56	This statement seems wrong. The positive phase of the NAO is associated with increased westerlies and mild winters over most of northern and central Europe and the negative phase with increased Scandinavian and Iceland blocking and an increased frequency of cold winter easterlies over much of Europe. Hence the 1960s winters (strongly negative	

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				NAOs) were cold over northern and central Europe, most famously winter 1962-3 which had the strongest negative NAO in the record. The 1990s winters were mainly very mild with a strong positive NAO. The actual pattern of positive NAO related surface temperature anomalies is close to that in: Scaife, A., J. Knight, G. Vallis and C.K. Folland, 2005: A winter surface climate response to changes in upper level atmospheric circulation. Geophys. Res. Lett. 32, L18715, doi: 10.1029/2005GL023226. Later statements at line 48 p55 et seq. are compatible with my comments and seemingly contradict the text criticised above. [Chris Folland]	
3-1439	A	46:54	46:54	SUGGESTION FOR A SUPPLEMENTARY STATEMENT: The blocking phenomena and the NAO, as planetary waves fluctuations, could be represented in the framework of the atmospheric regimes (see Ghill and Robertson, 2002: waves vs particles in the atmosphere's phase space: a pathway to long-range forecasting?. PNAS, vol 99, 2493-2500.) [Paolo Michele Ruti]	
3-1440	A	46:55	46:56	There is a mistake here. This should say that increased blocking over Europe is associated with negative NAO. [Nathan Gillett]	
3-1441	A	47:0	49:0	I confess that this line of discussion is very interesting to me, and to my own research, but I am concerned that this discussion is not really meant to to be digestable by anyone who is not a stratosphere/general-circulation specialist. I think that this discussion is far too cryptic to be an intergral part of the Observations section of this VERY long Chapter 3. Most of this discussion is not palatable to the other climate scientists who are writing in this Chapter 3, let alone the more eclectic "customers" of this AR4 IPCC Assessment. [Jerry Mahlman]	
3-1442	A	47:1		Don't need "(95% confidence level)" sine you define "significant" in Introduction. [Neville Nicholls]	
3-1443	A	47:2		As a co-author on Barriopedro et al. (2005), it is suggested that the following re-write be added: "Wiedenmann et al. (2002) did not find any long-term statistiaclly significant trends in NH block intensity, but, in the Pacific sector, Barriopedro et al. (2005) found that from 1948-2002 there was a significant increase in Western Pacific blocking days and events (57% and 62%, respectively). They also found statistically significant decreases in Atlantic Region blocking events and days, and these events were also less intense. Barriopedro et al. (2005) also show that there was a significant correlation between Atlantic Region blocking and the NAO, which supports the results of Quadrelli et al. (2001) and Sherrer et al. (2005). [Anthony Lupo]	

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3-1444	A	47:3		Following the Barriopedro discussion please consider adding: "Wiedenmann et al. (2002) found that there were slighly fewer blocking events in the NH during El Nino years, but that these events were significantly (at the 95% confidence level) weaker during these years. This is especially true within the Pacific Ocean basin region. [Anthony Lupo]	
3-1445	A	47:6	47:6	Spelling of 'Date Line' (correctly spelt here) [M James Salinger]	
3-1446	A	47:18	47:18	Insert "index" between "NAO" and "change" ? [Franco Desiato]	
3-1447	A	47:18	47:19	This conclusion is the opposite of what I would expect based on the usual definitions of blocking in the European area. The classical positive winter NAO pattern is associated with anomalous low pressure right across northern Europe as far as Russia, and high pressure anomalies over Southern Europe and the Mediteranean region which are not usually regarded as blocking. [Chris Folland]	
3-1448	A	47:18	47:21	In this summary on blocking trends, the text states “the NAO change over recent decades is dynamically consistent with decreases in blocking frequency over the West Atlantic and increases in blocking frequency over the European mainland” yet there has been no prior discussion about NAO changes. Are observed blocking changes consistent with the increase in the NAO from the 1960s through the late 1980s, or the decrease in the NAO from the early 1990s to present, or both? [Jeffrey Kueter]	
3-1449	A	47:23	48:20	This section starts with a discussion of the mean state of the stratosphere, and then goes on to discuss trends in wave activity, and finishes with a discussion of trends in geopotenial height. I would suggest that the trends in geopotential height, which are much more well known than trends in wave activity, are discussed after the discussion of the mean state, with trends in wave activity discussed last. [Nathan Gillett]	
3-1450	A	47:23		Section 3.5.5: Another example of a well written section! [Fons Baede]	
3-1451	A	47:23		Section 3.5.5. I am concerned that there is no mention of the QBO and only one in passing of the brewer-dobson circulation in this section. This seems incredibly odd and needs to be rectified if this section is to be considered in any meaningful way complete. [Peter Thorne]	
3-1452	A	47:30	47:31	"or wave activity changes in the distribution of wave drag within the stratosphere". I didn't exactly understand this, and anyway think this is superfluous - it is enough to say that the stratospheric circulation is driven by upward propograting planetary waves from	

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				the troposphere. [Nathan Gillett]	
3-1453	A	47:31	47:32	Perhaps before this sentence there should be a brief mention that ozone is formed predominantly in the tropics and then transported to the extratropics by the Brewer-Dobson circulation. [Nathan Gillett]	
3-1454	A	47:53	47:54	Radiosonde records extend before 1979, and these are not discussed. While they certainly have some failings in the stratosphere, there are also large uncertainties in the SSU data. [Nathan Gillett]	
3-1455	A	47:53	47:54	This statements ignores the existence of radiosonde data since the IGY. [Dian Seidel]	
3-1456	A	47:53	47:54	I disagree with this sentence in its generality. It is true for the extratropical southern hemisphere, but there is quite a lot of radiosonde data prior to 1979 in the northern hemisphere, and this is enough to characterise the sudden warmings, for example. Simmons et al. (2005) provide an illustration of the ERA-40 analysis of the 1958 warming that agrees well with contemporary manual analysis of radiosonde data, and of skilful numerical forecasts of the warming. Radiosonde observations depicting the QBO in the late 1950s (as used by Ebdon and Reed to discover the phenomenon) are also fitted well by the QBO in the ERA-40 analyses, as noted by Uppala et al. (2005). [Adrian Simmons]	
3-1457	A	47:57	48:1	"Observations show a downward trend in the NH wave forcing in the period 1979-2000" and in the previous paragraph "an incresaing trend of wave activity during winter has been reported after the late 1970s in both hemispheres". These statements are contradictory. Interannual variability in wave forcing is very large, and I suspect that the trends found are very sensitive to the method used to derive them. I suggest that the results of both studies are cited together and an assessment that the trends in wave activity are rather uncertain is included. [Nathan Gillett]	
3-1458	A	47:57		If you really need to mention Brewer-Dobson, you need a reference, if not a definition. [Neville Nicholls]	
3-1459	A	48:6	48:6	What are the years that were used in the Thompson and Solomon study? [Steven Massie]	
3-1460	A	48:9	48:9	I think 'autumn' should be 'spring' here. According to the radiosonde temperature trends shown by Thompson and Solomon (2002) the main stratospheric cooling has occurred in the boreal spring and summer.	

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				[Nathan Gillett]	
3-1461	A	48:24	49:16	Box 3.2. The Scaife et al (2005) paper is relevant here too. There is arguably too much detail not directly relevant to climate change, including Fig 3.5.2. Or the relevance needs bringing out more clearly. [Chris Folland]	
3-1462	A	48:24	49:16	Suggest adding following comment somewhere in this section on strat-trop relations: Scaife et al. (2005) have demonstrated in the MO climate model that including a realistic representation of the stratospheric circulation, and hence allowing the downward coupling of observed stratospheric changes to the surface, improves the representation of the changes in the NAO strength between 1965 and 1995. Scaife AA, Knight JR, Vallis GK, Folland CK. A stratospheric influence on the winter NAO and North Atlantic surface climate. GEOPHYSICAL RESEARCH LETTERS 32 (18): Art. No. L18715 SEP 28 2005 [Lesley Gray]	
3-1463	A	48:26		The authors could mention that these waves are Rossby waves. [Nathan Gillett]	
3-1464	A	48:35	48:37	Prior to cited references, Yamazaki and Shinya (1999) clearly shows that the northern annular mode is an internal mode by a perpetual winter AGCM simulation and the mode arises from wave-zonal flow interaction. I suggest to add Yamazaki and Shinya (1999) into the citation. Yamazaki, K. and Y. Shinya, 1999: Analysis of the Arctic Oscillation simulated by AGCM. J. Meteor. Soc. Japan, 77, 1287-1298. [Koji Yamazaki]	
3-1465	A	48:57		In addition to Baldwin et al. (2003), this recent paper is also relevant and should also be cited here: Charlton, A.J., A. O'Neill, D.B. Stephenson, W.A. Lahoz, and M.P. Baldwin 2003: Can knowledge of the state of the stratosphere be used to improve statistical forecasts of the troposphere?, Quarterly Journal of the Royal Meteorological Society, 129, 3205-3224. [David Stephenson]	
3-1466	A	49:22		section 3.5.6 Winds, waves and surface fluxes The whole topic of wind wave fields reconstruction using models and analysed wind fields has not been considered. I think that it should be mentioned. I suggest to insert the following text at end of page 49, after line 57: "Besides visual observations, wave data are provided by buoys and satellites, but the available time series are generally too short for evaluating multi-decadal variability and	

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				trends. Model reconstructions, using wind forcing fields provided by meteorological reanalysis, have been used as alternative sets of data. An example is the analysis of SWH (Significant Wave Height) field in the Mediterranean Sea, in the period 1958-2001 (Lionello and Sanna, 2005) based on model simulations forced by the wind fields of the ERA-40 (ECMWF Re-Analysis). A statistically significant negative trend has been found in the winter mean value (-.2cm/y) which is associated to weakening north-westerly (Mistral) winds. Wind wave extremes show significant trends only in small regions: a negative trend in part of the central and western Mediterranean (Ionian Sea and Alboran Sea) and a positive trend near the coast of France (Lionello et al. 2005" The references are: Lionello P. and A.Sanna (2005) Mediterranean wave climate variability and its links with NAO and Indian Monsoon Clim.Dyn. DOI: 10.1007/s00382-005-0025-4 Lionello P., Bhend J., Buzzi A., Della-Marta P.M., Krichak S., Jansà A., Maheras P., Sanna A., Trigo I.F., Trigo R. (2005). Cyclones in the Mediterranean region: climatology and effects on the environment. In P.Lionello, P.Malanotte-Rizzoli, R.Boscolo (eds) Mediterranean Climate Variability. Amsterdam: Elsevier (NETHERLANDS). in press [Piero Lionello]	
3-1467	A	49:22		Please could you ask the writers of section 3.5.6 on waves to consider the few references in the following text I had sent to the authors of chapter 5 but seem not to have been copied over to chapter 3? The TAR reported increasing North Atlantic wave heights during the past half century, much of the variability in wave height related to fluctuations in the North Atlantic Oscillation (e.g. Bacon and Carter, 1993). Numerical wave modelling exercises have confirmed this general picture, although at many locations studied trends are only weakly positive (Vikeb¿ et al. 2003). Lozano and Swail (2002) discussed the relationship between North Atlantic wave heights and storm tracks during the last four decades. Similar evidence exists for increasing eastern North Pacific wave heights during the past 20-30 years (Allan and Komar, 2000). As many flooding events at low-lying islands are actually caused by swell rather than sea level change (e.g. at the Maldives, Harangozo 1992), the potential modification of waves and swell as a consequence of climate change, is clearly an issue which needs attention, alongside the more intensively-studied topics of changes in mean sea level and storm surges (Vassie et al. 2004). Harangozo, S.A., 1992: Flooding in the Maldives and its implications for the global sea level rise debate. pp.95-99 in, Sea level changes: determination and effects, (ed. P.L.Woodworth, D.T.Pugh, J.G.De Ronde, R.G.Warrick & J.Hannah). Washington, DC: American Geophysical Union. 196pp. Lozano, I. and Swail, V. 2002. The link between wave height variability in the North	

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				Atlantic and the storm track activity in the last four decades. Atmosphere-Ocean, 40, 377-388. Vassie, J.M., Woodworth, P.L. & Holt, M.W. 2004 An example of North Atlantic deep ocean swell impacting Ascension and St. Helena islands in the central South Atlantic. J. Atmos. Ocean. Tech., 21(7), 1095-1103. Vikeb¿, F., Furevik, T., Furnes, G., Kvamst¿, N.G. and Reistad, M. 2003. Wave height variations in the North Sea and on the Norwegian continental shelf, 1881-1999. Continental Shelf Research, 23, 251-263. [Philip Woodworth]	
3-1468	A	49:24	50:22	Several types of measurement, direct or indirect, related to the estimate of ocean-surface conditions are not mentioned. See comment 51 for some of the data types. [Adrian Simmons]	
3-1469	A	49:26		Insert ‘about’ or ‘around’ before ‘150’ [Ian Simmonds]	
3-1470	A	49:32	49:33	I question the final words "will likely dominate". "dominate" is too strong. I think it would be better to say "With ICOADS data assimilated in reanalyses, inhomogeneities in the raw VOS data will be refleflected to some degree in inhomogeneities in reanalysis products". How much depends on how other data influencing marine wind speeds are assimilated - the amount of such data and the error characteristics of the data and assimilating model. Surface-pressure data (used in a modern multi-variate analysis system) certainly will certainly influence surface wind analyses, and an increasing number of types of satellite data also will from about 1979 onwards, when influence of low-level winds estimates from geostationary orbit will spread down to the surface (followed in later years by more direct data influences from SSM/I and scatterometers, and indirect influences from altimeter and SAR measurements of ocean-wave conditions. There's also ocean buoy data, which also need to be used at the correct height; this was not, incidentally, the case for the TAO moored array in ERA-40, or in ECMWF operations until August this year! [Adrian Simmons]	
3-1471	A	49:33	49:37	Doesn't make sense - something missing? [Robert E. Dickinson]	
3-1472	A	49:38	49:40	I strongly doubt that "Visual VOS observations of wind waves … have been less affected by changes in observational practice". The increased amount of traffic, and the increased reliability and size of ships is likely to have had a major effect on the extreme SWH that have been recorded. It is difficult to evaluate this effect, but the quoted statement looks exceedingly confident and should be deleted or modified. I suggest: "Visual VOS observations of wind waves … are an important source of information, though it is	

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				difficult to evaluate how much they have been affected by increased marine traffic and reliability and size of ships." [Piero Lionello]	
3-1473	A	49:38	49:57	Three more references, "Wang and Swail, 2005, 2002, and 2001" (see Comments #15-17 below), should be cited in this paragraph, because these studies assess historical changes in significant wave heights (SWH) in the NH oceans using the best hindcast data available to date (see the next two comments). [Xiaolan L. WANG]	
3-1474	A	49:40	49:42	The whole sentence "Wind speed….. Magnitude" is not well formulated. Windsea is the part of the wave spectrum generated by the local wind, swell the part produced by a distant storm. I do not understand the statement that swell is influenced by the frequency of storms, because the swell amplitude and period depend on the intensity of the storm which produced it and the distance travelled by the waves. I suggest to replace this sentence with "The local wind speed affects directly the windsea component of the wave spectrum, while swell is the results of a generation which took place in an eventually remote area, so that the swell regime would be affected by the frequency and intensity of remote storms." Otherwise, the whole sentence could be deleted as the information is actually not needed. [Piero Lionello]	
3-1475	A	49:47	49:48	Suggest replace these two lines with "... and hindcast data (Wang and Swail, 2005 and 2001; Graham and Diaz, 2001), although primarily negative trends were found in wind and seasonal mean and extreme SWH in the region off the west coast of Canada (Tuller, 2004; Wang and Swail, 2005 and 2001). In the Atlantic, ..." (see Comments #15 and #17 below) [Xiaolan L. WANG]	
3-1476	A	49:56	49:56	14-year time series is too short in order o confirm a statement; add the uncertainty related to this aspect. [Aristita Busuioc]	
3-1477	A	49:57	49:57	Replace "(Woolf et al., 2002)." with "(Woolf et al., 2002) and was found to occur mainly in summer and fall (Wang and Swail, 2005, 2002, and 2001)." (see Comments #15-17 below) [Xiaolan L. WANG]	
3-1478	A	50:4	50:16	Referring to local/regional "changes" is more honest than inferring "trends". [Jerry Mahlman]	
3-1479	A	50:10	50:12	The sentence beginning 'Recent evaluations…' is incorrect because it implies that Grist	

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				and Josey (2003) find biases in the global mean net heat flux in the various air-sea flux products (including both reanalysis and in situ observation based climatologies) they considered of several tens of W m–2. In fact, of the products considered, a bias of this magnitude is only present in the original SOC fluxes, in contrast the fluxes from the reanalyses are close to being balanced. Coupled models were not considered by Grist and Josey (2003) but it is now well established that various coupled models (e.g.HadCM3) have global mean net heat flux close to zero i.e. they do not have biases of several tens of Wm-2.As it stands, the sentence misrepresents a.) the general situation regarding global flux biases as reanalyses and coupled models do not exhibit biases of several tens of Wm-2 and b.) the specific work of Grist and Josey (2003) who found biases in unconstrained VOS products but did not find global biases in reanalyses and coupled models. It should be replaced with the following: 'Recent evaluations of heat flux estimates from reanalyses and in situ observations indicate some improvements but there are still global biases of several tens of W m–2 in unconstrained VOS observation based products (Grist and Josey, 2003).' Note I have raised this point (jointly with Sergey Gulev) separately with Kevin Trenberth and he agrees that the suggested revision is needed. [Simon Josey]	
3-1480	A	50:12	50:13	I disagreed with a few statements on topics that I am familiar with. 1. I am sure that the statement "Only indirect estimates of the net from atmosphere to ocean heat budgets give reasonable implied ocean heat transports (Trenberth and Caron, 2001)" on lines 12-13 of page 3-50 would not be acceptable to Contributing Author Simon Josey, for the Grist and Josey adjusted estimates of air-sea heat exchange or indeed the adjusted da Silva et al.'s estimates accounting for global biases give reasonable ocean heat transports. I admire the Trenberth and Caron estimates, but not to the exclusion of other responsible estimates. [Harry Bryden]	
3-1481	A	50:12	50:13	The sentence beginning 'Only indirect…' is incorrect because it states that it is only possible to get reasonable estimates of the implied OHT using 'indirect estimates' which we take to mean residual method calculations. In fact, both the original NCEP/NCAR reanalysis fluxes (see Grist and Josey, 2003, Fig.9) and some coupled models (e.g. HadCM3 and NCAR CSM, see Trenberth and Caron, 2001, Fig.6) also provide implied ocean heat transports which are reasonable.The sentence should be replaced with 'Estimates of the implied ocean heat transport from the NCEP/NCAR reanalysis, indirect residual techniques and some coupled models (e.g. HadCM3, NCAR CSM) are in reasonable agreement with hydrographic observations (Trenberth and Caron, 2001; Grist and Josey, 2003).' As above, I have raised this point (jointly with Sergey Gulev) separately with Kevin Trenberth and he agrees that the suggested revision is needed.	

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				[Simon Josey]	
3-1482	A	50:19	50:21	For lines 19-21 on page 3-50, I think a reference to Josey and Marsh (2005) is missing [Harry Bryden]	
3-1483	A	50:19	50:21	This finding needs explanation as the strong positive NAOs frequent in the last 20 years and peaking in the 1990s are associated with anomalous cold north westerly surface advection over the Labrador sea. [Chris Folland]	
3-1484	A	50:24	50:31	Paragraph 3.5.7 is summary for all 3.5 ?, It is welcome, but the previous paragraphs had not such summary. [ILEANA MARES]	
3-1485	A	50:26	50:27	The summary should describe sinthetically the content of section 3.5. The reference to the successive section 3.6 seems inappropriate. [Franco Desiato]	
3-1486	A	50:26	:31	What happens after late 90ies? Should be mentioned even if unclear.. [Gabriele Hegerl]	
3-1487	A	50:30	50:31	Again, these are not likely to be "trends" in the true IPCC climate-warming sense. [Jerry Mahlman]	
3-1488	A	50:33		This section has fantastic background information but it is a bit weak on the "change" part. Can't we really say something more about observed changes in circulation variability modes? [FILIPPO GIORGI]	
3-1489	A	50:33		Again on this topic.I did not see anything in the chapter about changes in interannual variability (not specifically related to different modes), say as would be measured by interannual standard deviation. Is there nothing available on this? [FILIPPO GIORGI]	
3-1490	A	50:33		Much of this section on modes of variability could be removed as it is tutorial in nature. I don't think this report needs to serve the function of a primer on climate modes. It is enough to review recent thinking on the limited number of modes needed to encompass most of the variability observed and the challenge of disentangling trends from these modes. I can understand, though, that others may find value in this material. In the interest of brevity though, this is a good candidate for drastic editing. [Dian Seidel]	
3-1491	A	50:37	50:53	I suggest distilling the first two paragraphs and removing text that is repeated in paragraph 4 of this section: "A number of preferred patterns of variability exist in the global atmospheric circulation, all of which have expressions in surface climate. Box 3.3 discusses the main patterns and indices used here. Regional climates in different locations	

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				may vary out of phase, due to the action of such "teleconnections" (see also Section 3.5). Understanding the nature of teleconnections and changes in their behaviour are central to our understanding of climate variability and change, as they encapsulate much of the seasonal and longer-timescale variability in the troposphere and determine, to a large extent, regional surface climate anomalies. Such anomalies have direct human impacts, often being associated with droughts, floods, heat waves and other changes that can severely disrupt agriculture and fisheries, and can modulate air quality, fire risk, energy demand and supply, and human health." [Richard Allan]	
3-1492	A	50:42	50:42	"annular mode SAM" is redundant [Dian Seidel]	
3-1493	A	50:46		The following recent review of teleconnection patterns also deserves to be cited: Panagiotopoulos, F., M. Shahgedanova, D.B. Stephenson, 2002: A Review of Northern Hemisphere winter-time teleconnection patterns, (European Research Course on Atmospheres ERCA: - Vol. 5 Ed. C. Boutron), J. Phys IV, 12, 1027-1047, EDP Sciences, France. The following recent review of teleconnection patterns also deserves to be cited: Panagiotopoulos, F., M. Shahgedanova, D.B. Stephenson, 2002: A Review of Northern Hemisphere winter-time teleconnection patterns, (European Research Course on Atmospheres ERCA: - Vol. 5 Ed. C. Boutron), J. Phys IV, 12, 1027-1047, EDP Sciences, France. [David Stephenson]	
3-1494	A	50:48	50:53	Background information could be deleted. [Neville Nicholls]	
3-1495	A	51:1	51:6	SUGGESTION FOR A SUPPLEMENTARY STATEMENT: The appearance of uni- or multimodal behaviour in the atmospheric flows could be related to the strength of the upper tropospheric jet (Ruti, P.M., V. Lucarini, A. Dell'Aquila, S. Calmanti, A. Speranza, 2005: "Does the subtropical jet catalyze the mid-latitude atmospheric regimes? ", GRL, revised submission.). [Paolo Michele Ruti]	
3-1496	A	51:1		The authors fail to mention that this non-linear interpretation is not a consensus view and is subject to an ongoing debate. The existence or not of multiple climate regimes has been discussed at more length in the recent paper: Stephenson, D.B., A. Hannachi, and A. O'Neill, 2003: On the existence of multiple climate regimes, Quarterly Journal of the Royal Meteorological Society, 130, 583-605.	

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				[David Stephenson]	
3-1497	A	51:3	51:6	Conceptually, the preferred states, as naturally occurring expressions of the dynamics, are likely to be relatively robust to modest changes in forcings (not climate… modest changes in forcings can cause -in principle- big changes in climate!). Furthermore, when one investigates changes in regime properties as a function of anomalies in forcing, two possibilities should be considered. If the forcing variations are relatively small, then the number of distinct preferred states (or regimes) is likely to remain the same. In this case, one should mainly be concerned with changes in the frequency of a given set of regimes (as argued Palmer, 1999). Strong forcing variations, on the other hand, can take non-linear systems through bifurcation points, and the number of flow regimes can be altered (e.g. Ghil 1987 [Ghil, M., 1987: Dynamics, statistics and predictability of planetary flow regimes, Irreversible Phenomena and Dynamical Systems Analysis in the Geosciences, C. Nicolis and G. Nicolis (Eds.), D. Reidel, Dordrecht/Boston/Lancaster, pp. 241-283)]. Variations in the number of regimes caused by strong ENSO events have been reported by Molteni and Corti (1998) [Molteni F. and Corti S., 1998: Long term fluctuations in the statistical properties of low-frequency variability: dynamical origin and predictability. Q. J. R. Meteorol. Soc. 124, 495-526], Straus and Molteni (2004) [Straus, D. and Molteni F., 2004: Circulation regimes and SST forcing: Results from large GCM ensembles. J. Climate, 17, 1641-1656] and Molteni et al. (2003) [Molteni, F., Corti S., Ferranti L. and Slingo J.M., 2003: Predictability Experiments for the Asian Summer Monsoon: Impact of SST Anomalies on Interannual and Intraseasonal Variability. J. Climate., 16 , 4001-4021] in modelling studies of the extratropical and tropical circulation, respectively [SUSANNA CORTI]	
3-1499	A	51:10	51:10	The appearance of Fig. 3.6.* in this line is somewhat confusing. Is it possible to change the order of those figures? [kyung-ryul Kim]	
3-1500	A	51:13	51:15	Not sure this sentence adds much. [Neville Nicholls]	
3-1501	A	51:13	:15	This sentence needs rewriting since it is not clear what it means. Long-range persistence and its consequences for multi-decadal trending in the NAO was discussed in detail in Stephenson, D.B., V. Pavan, and R. Bojariu, 2000: Is the North Atlantic Oscillation a random walk?, International Journal of Climatology, 20, 1-18. [David Stephenson]	

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3-1502	A	51:17	51:32	See page 50, comment above. [Referring to local/regional "changes" is more honest than inferring "trends". Again, these are not likely to be "trends" in the true IPCC climate-warming sense.] Again, the language of "variability vs trends" needs to be clarified. Much of 3.5 appears to be more about "variability" than about "true secular trends". All of this needs some research-level thinking and rewriting to insure that we are not hopelessly confusing the interested, non-specialist, reader. [Jerry Mahlman]	
3-1503	A	51:30		Change "Researchers must" to "It is necessary to". [Adrian Simmons]	
3-1504	A	51:40	51:57	This could be shortened substantially. Too much background information. [Neville Nicholls]	
3-1505	A	51:43		Change "that" to "which" [Adrian Simmons]	
3-1506	A	51:49	:57	This discussion on station vs pattern indices is nice and could also refer to the same issue for the NAO, where the station based index may be more homogeneous but also more noisy [Gabriele Hegerl]	
3-1507	A	51:50		In addition to smoothing, one should also be careful as to how one removes time-varying annual cycles in order to make indices. Recent work shows that careful seasonal adjustment rather than the crude assumption of a fixed annual cycle can lead to improved ENSO indices: Pezzulli, S., D.B. Stephenson, and A. Hannachi 2005: The Variability of Seasonality, J. Climate, 18, 71-88. This type of approach is likely to become ever more necessary as annual cycles change due to global warming. [David Stephenson]	
3-1508	A	52:1	57:1	Would it be possible to add cartoon-style figures illustrating in conceptual terms how climate changes for high and low NAM, SAM, and ENSO states? I hesitate to suggest this given length constraints but there is a great deal of excellent material on the modes of variability here, and at least one illustrative figure (maybe for the NAM, given the large trend?) would be useful for the reader. [Susan Solomon]	
3-1509	A	52:4	52:27	To be corformand consequent, it should be discerned between processes like SO, NAO etc. and the indices like SOI, NAOI etc. which quantify these processes by means of appropriate standardized measures like MSLP gradients (also in the following). Moreover I wonder that the Arctic Oscillation (AO) is not addressed. [Christian-D. Schoenwiese]	

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3-1510	A	52:12		Add "this series" after "but" [Adrian Simmons]	
3-1511	A	52:17		Gong and Wang used 40oS, not 45oS [Ian Simmonds]	
3-1512	A	52:23	52:27	Include the definition of the IPO in this section, and how it relates to the PDO. [Chris Folland]	
3-1513	A	52:34	53:33	I think this too much detail on ENSO - reads like a textbook, and ignores the possibility of providing a simple description with a single reference to provide background information. [Neville Nicholls]	
3-1514	A	52:43		This is an example where a reference to a Trenberth paper could be replaced by a different publication. As the reference here is qualifying a quite general statement about the fundamental nature of El Nino, not a recent post-TAR/AR3 finding, perhaps here an older reference or two can be given. There may be an IPCC policy to reference papers from the post-TAR/AR3 period as far as possible, but this brings a danger of giving the impression that time is being spent reinventing the wheel. [Adrian Simmons]	
3-1515	A	53:7	53:8	SUGGESTION FOR A SUPPLEMENTARY STATEMENT: However, non-linearities play a relevant role in determining the mid-latitude response to the ENSO forcing (A. Hannachi, 2001: " Toward a nonlinear identification of the atmospheric response to ENSO". J. of Climate, 14, 2138-2149). [Paolo Michele Ruti]	
3-1516	A	53:8	53:8	However, non-linearities play a relevant role in determining the mid-latitude response to the ENSO forcing (see for example Wu and Hsieh 2004 [The nonlinear association between ENSO and the Euro-Atlantic winter sea level pressure CLIMATE DYNAMICS 859-868], Wu, Hsieh and Shabbar 2005 [The nonlinear patterns of North American winter temperature and precipitation associated with ENSO, Journal of Climate, 18, 1736-1752] [SUSANNA CORTI]	
3-1517	A	53:10	53:10	Figure 3.6.2 would benefit from adding an updated Ninox SST index, something like that in the TAR. [Chris Folland]	
3-1518	A	53:10	53:10	. Misleading figure, inaccurate text. "El Nino" is defined as a pressure anomaly that is one standard deviation away from the long term mean. Here, ANY deviation in Darwin pressure is defined as an El Nino. The definition of one of the world's great circulation systems should not be changed. [Jeffrey Kueter]	

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3-1519	A	53:13	53:14	Reference should also be made to the very strong El Nino events of the late 19th century (1877-78 and 1888-89) that were a trigger for basic research into variability of tropical monsoon rainfall and the Southern Oscillation. The work of Walker, etc is cited. [WILLIAM KININMONTH]	
3-1520	A	53:16	53:16	Worth expanding on the character of the change in El Nino evolution around 1976. Is there any evidence in the instrumental record of an earlier period with the current typical El Nino evolution? [Chris Folland]	
3-1521	A	53:27	53:27	Global mean what? Temperature I assume. [Peter Thorne]	
3-1522	A	53:28	53:28	as compared to? [Qiang Fu]	
3-1523	A	53:28	53:30	Good point. The question extrapolates importantly to the quest to identify "true secular trends". [Jerry Mahlman]	
3-1524	A	53:31	53:33	The sentence spanning these line should go, or be completely rewritten. It follows the words "enhanced by global warming" at the end of the previous sentence, and appears to be presented as an example of a single climatological event that was enhanced by global warming. This is at odds with what is written later (Page 73, line 14). Drought in Australia, 2002-2003, gets its own box on page73, so the questionable reference to it on page 53 can be deleted without loss to the chapter as a whole. [Adrian Simmons]	
3-1525	A	53:41	53:45	Fig 3.6.1: Although not critical, this particular one point correlation map extends the anomalous strongest positive winter NAO pressure anomalies east of many other definitions, so may not be best. My preference is for the Hurrell et al (2003) EOFs (Fig 6), especially as they can be used for describing the seasonal cycle of NAO-like patterns and link directly to Fig 3.6.6. [Chris Folland]	
3-1526	A	53:50		is" instead of "are [Neville Nicholls]	
3-1527	A	53:53		delete "considered" [Neville Nicholls]	
3-1528	A	54:1	54:12	Again, too much background information. [Neville Nicholls]	
3-1529	A	54:12		insert “Notably, Fogt and Bromwich (2006) demonstrate that the center of action in the PSA mode in the South Pacific is overlaps a center of action for the high southern latitude	

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				circulatin captured by the SAM. Their study found that when both oscillations are in phase, the geopotential height and pressure response in the South Pacific is amplified. Further, they demonstrated that the coupling between these two oscillations leads to a pronounced decadal variability in the high southern latitude ENSO teleconnection, most marked between the 1980s and 1990s in SON. This decadal variability of the South Pacific ENSO teleconnection influenced the moisture flux onto West Antarctica [i.e, Bromwich et al. 2000], as well as the circulation anomalies in the Antarctic Peninsula and southern South America, leading to marked decadal climate changes in these regions." [David / Ryan Bromwich / Fogt]	
3-1530	A	54:14	54:56	3.6.3. Again, "modes" vs. the real goals of true trend identification needs to be clarified, and highlighted. [Jerry Mahlman]	
3-1531	A	54:14	55:2	Section 3.6.3 as a whole: Include a mention of the observational results of McPhaden and Zhang (2002) on the context of PDO/IPO mechanisms or the Pacific climate shift of 1976/77, or both, especially the atmospheric results. McPhaden, M.J. and D. Zhang, 2002: Slowdown of the meridional overturning circulation of the upper Pacific ocean. Nature, 415, 603-608 [Chris Folland]	
3-1532	A	54:20	54:23	Same comment as above (22). [Christian-D. Schoenwiese]	
3-1533	A	54:22	54:23	The IPO SST pattern does not agree in all respects with Fig 3.6.3 in the tropics. The IPO pattern 3.6.3 especially puts more emphasis on Southern Hemisphere negative SST anomalies, more comparable to the Northern Hemisphere and extends the postive anomalies further west. Either show this pattern or mention IPO differences from Fig 3.6.3 (use Folland et al, 1999 or Folland et al, 2002 as in the refs). [Chris Folland]	
3-1534	A	54:25	54:32	Section 3.6.3 The Amazon region also shows decadal variability,and rainfall variability in northern and southern Amazonia show shiftd in the middle 1940's and 1970's (Marengo 2004) [Jose Marengo]	
3-1535	A	54:25	54:32	A decadal scale cooling of the upper troposphere dominates East Asia, resulting in the South Flood and North Drought decadal anomalous rainfall pattern in China, the July to August westward retraction of the cooling signal is affected by the PDO. See: Yu Rucong, Tianjun Zhou, and Xiaoge Xin, 2005, Atmospheric cooling trend and the climate change over East Asia in the past 50 years, Journal of Climate, submitted [Rucong Yu]	
3-1536	A	54:25	54:32	Recent diagnosis reveals a decadal scale cooling of the upper troposphere over East Asia,	

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				which dominates the South Flood and North Drought decadal anomalous rainfall pattern in China, the July to August westward retraction of the cooling signal is affected by the PDO. For reference, see: Yu Rucong, Tianjun Zhou, and Xiaoge Xin, 2005, Atmospheric cooling trend and the climate change over East Asia in the past 50 years, Journal of Climate, submitted [Tianjun ZHOU]	
3-1537	A	54:41		Is there convincing evidence that this bi-decadal rhythym is real, and not just the result of sampling an autocorrelated series over a short period? [Nathan Gillett]	
3-1538	A	54:45	54:45	...similar to the paradigm for ENSO (Knutson and Manabe, 1998; Evans et al., 2001…) Reference to add: Knutson, T. R., and S. Manabe, 1998: Model assessment of decadal variability and trends in the Tropical Pacific Ocean. Journal of Climate, 11(9), 2273-2296. [Thomas Knutson]	
3-1539	A	54:46		What is a "decadal-to-interdecadal" timescale? I think a timescale has to be either decadal, multi-decadal or sub-decadal, not interdecadal. [Adrian Simmons]	
3-1540	A	54:48	54:48	remove sentence about the extratropical influence of the ocean to ENSO. Reference to more recent work should be made here about the influence of off equatorial winds to the shallow overturning circulation, transport of water to the equatorial thermocline and the potential influence on ENSO. An important reference that is missing is McPhaden and Zhang(2002) and McPhaden and Zhang (2004), please see the references in Chapter 5 for the complete references and refer to chapter 5 page 14 for a complete discussion. This work really represents the state of the field. [LuAnne Thompson]	
3-1541	A	54:54	54:54	chance low frequency? I do not understand [Bernard Seguin]	
3-1542	A	54:54	55:1	Vivès and Jones (2005) have identified step changes in decadal mean rainfall in Australia, downwards in the early 1890s and upwards in 1948 that appear to be coincident with such oscillations. Vivès, B. and R.N. Jones (2005) Detection of abrupt changes in Australian decadal rainfall (1890–1989), CSIRO Atmospheric Research Technical Paper No. 73, 54 pp. [Roger Jones]	
3-1543	A	55:0		The Northern Annular Mode (NAM, subchapter 3.6.4) is widely known in literature as Arctic Oscillation (AO). I suggest that the term AO might also be mentioned in the report. [Jaak Jaagus]	
3-1544	A	55:0		Repeated reference is made to the NAO trend from the 1960s to the early to mid 1990s, when clearly Figure 3.6.6 shows that it peaked in the late 1980s to about 1990 and has	

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				been declining since—annual values, as well as the low pass filter, both depict this behavior. [Jeffrey Kueter]	
3-1545	A	55:8	55:21	Mention the seasonal cycle of the change in the NAO pattern, particularly from winter to summer. Barnston and Livezey (1987) and Hurrell et (2003), Fig 6, give a fairly similar story. [Chris Folland]	
3-1546	A	55:8	56:30	I think there should be some linkage of this section to the previous section on p48 on strato-trop interaction - and a reference made to the Scaife et al. 2005 paper (see ref above). [Lesley Gray]	
3-1547	A	55:8	56:7	Because to NAO is dedicated a large space in this paragraph, I suggest to diminish the well- known description of NAO influence and also to introduce a recent reference: McDaniel B. A. and R.X. Black, 2005: Intraseasonal Dynamical Evolution of the Northern Annular Mode. J. Climate, 18, 3820-3829. The authors concluded that low-frequency planetary-scale waves provide the primary dynamical forcing for short-term intraseasonal variations of the NAM in both the stratosphere and troposphere. [ILEANA MARES]	
3-1548	A	55:8	59:14	Yet again, the quest for separability of interannual/interdecadal "modes" of variability or decadal/century-scale trends needs careful attention here, with the possible bonus of shortening this massively lengthy discussion over multiple sections. As of now, "mode think" dominates the Chapter 3 analysis of the bottom line: Just what aspects of all of this are teaching us about the science of global warming? We do have to remain highy focussed on the purpose of these IPCC assessments. Just what is it that we now know about global warming, and how is it manifested on regional scales, and can we intelligently separate out transient regional temperature "trends" from true regional-scale, global-warming-driven trends? I think it is fair to say that this Chapter does not yet speak meaningfully to this compellingly important question. [Jerry Mahlman]	
3-1549	A	55:8		Section 3.6.4. This is an excellent section. At the end it gives some useful numbers for the contribution of the NAM to Eurasian T changes. My concern is that the contributions to NH T trends are not explored elsewhere. Most of the global mean T change arises from land areas and Eurasia is a very large part of the total land area. There exists the possibility that a significant fraction of the NH T trend is due to the NAO. Can this be estimated?. It should at least be mentioned. [Kenneth Carslaw]	
3-1550	A	55:21	:23	The NAO/NAM debate was discussed in detail in the highly cited paper:	

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				Ambaum, M.H.P., B.J. Hoskins, and D.B. Stephenson, 2001: Arctic Oscillation or North Atlantic Oscillation?, J. Climate, 14, 3495-3507. and the corrigendum: Ambaum, M.H.P., B.J. Hoskins, and D.B. Stephenson, 2002: Corrigendum: Arctic Oscillation or North Atlantic Oscillation?, J. Climate, 15, 553. These recent papers SHOULD be cited. [David Stephenson]	
3-1551	A	55:23	55:23	Concerned the NAO-AO-NAM debate, there is a number of paper that should be quoted. For example i) Ambaum, M.H.P., Hoskins, B., and Stephenson, D.B.: 2001, Arctic oscillation or North Atlantic oscillation?, J. Climate 14, 3495–3507. ii) Wanner et al., 2001: North Atlantic Oscillation – concepts and studies. Survey in geophysics, 22, 321-382 [SUSANNA CORTI]	
3-1552	A	55:23	55:24	Emphasis you are talking about the winter NAO and NAM. [Chris Folland]	
3-1553	A	55:23	55:24	"the time series are highly correlated" -I suggest to give a correlation coefficient if is possible [ILEANA MARES]	
3-1554	A	55:23	55:24	SUGGESTION FOR A SUPPLEMENTARY STATEMENT: In this debate, two papers should be quoted: i) Ambaum, M.H.P., Hoskins, B., and Stephenson, D.B.: 2001, Arctic oscillation or North Atlantic oscillation?, J. Climate 14, 3495–3507. ii) Wanner et al., 2001: North Atlantic Oscillation – concepts and studies. Survey in geophysics, 22, 321-382. [Paolo Michele Ruti]	
3-1555	A	55:25	55:28	Two studies may be relevant in this context and may possibly be referred to along with Weaver and Nigam (2000; WN). (1) Kimoto et al. (2001) presented a neutral mode theory for AO quoting zonal-eddy interaction as the maintenance mechanism, in accordance with the analysis by Weaver and Nigam (2000). (2) Watanabe and Jin (2004) further updated the theory to include transient-eddy feedback. Kimoto, M., F.-F. Jin, M. Watanabe, and N. Yasutomi, 2001: Zonal-eddy coupling and a neutral mode theory for the Arctic Oscillation. Geophys. Res. Lett., Vol. 28, No. 4, 737-740. Watanabe, M. and F.-F. Jin, 2004: Dynamical prototype of the Arctic Oscillation as	

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				revealed by a neutral singular vector. Journal of Climate, 17, 2119-2138. [Masahide Kimoto]	
3-1556	A	55:33	55:33	NAO values have not declined marginally, but significantly. They have returned to zero (average for the past five years). [Stefan Brönnimann]	
3-1557	A	55:34	55:36	Besides the Rodwell (2003) ref, there is a recent journal reference to the summer NAO trend which contains observational as well as model results, so can be quoted here: Rodwell, M.R. and C.K. Folland, 2003: Atlantic air-sea interaction and model validation. Annals of Geophysics, 46, 47-56. See Fig 1 and p52. [Chris Folland]	
3-1558	A	55:36	55:37	The statement "The increase in the trend and variance of the NAO/NAM over roughly the last three decades is greater than would be expected from climate noise alone" is incorrect. See for example the data presented at: http://www.cpc.ncep.noaa.gov/products/precip/CWlink/pna/month_nao_index.shtml The statement is even more misleading for the winter NAO. [Jeffrey Kueter]	
3-1559	A	55:38	:38	This statement is only true if one assumes (incorrectly) as Feldstein did that climate noise is an AR(1) process. Stephenson et al. (2000) showed that such stochastic trends could easily result from the natural long-range dependence in the NAO series: Stephenson, D.B., V. Pavan, and R. Bojariu, 2000: Is the North Atlantic Oscillation a random walk?, International Journal of Climatology, 20, 1-18. [David Stephenson]	
3-1560	A	55:39	55:40	Insert 'simulated' before 'atmospheric internal variability'. [Nathan Gillett]	
3-1561	A	55:40	55:41	"There may be predictability from stratospheric influences". It seem to be worth adding a comment, that recent studies on the predictability of the NAO show a weak but significant skill for the last 15 winters but no skill if the last 40 years are considered. Reference: Muller WA, Appenzeller C, Schar C Probabilistic seasonal prediction of the winter North Atlantic Oscillation and its impact on near surface temperature CLIMATE DYNAMICS 24 (2-3): 213-226 FEB 2005 [Christof Appenzeller]	
3-1562	A	55:40	55:46	The new Scaife et al (2005) paper is relevant here too. [Chris Folland]	

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3-1563	A	55:40		The wording "Although sub-seasonal atmospheric events appear to be largely non-predictable" has to go, or be reworded. Sub-seasonal atmospheric events include weather events, and these certanly have a degree of predictability! Forecasts out to a month ahead are showing some skill for at least the first three weeks. Is the statement in question intended to refer to events with a timescale of a month or two? Also use "unpredictable" not "non-predictable". [Adrian Simmons]	
3-1564	A	55:48	55:50	The surface air temperature over the subtropical Eurasia continent, eastward to the downstream of the Tibetan Plateau, are significantly correlated with NAO variability. Reference: Yu Rucong, Tianjun Zhou, 2004, Impacts of winter-NAO on March cooling trends over subtropical Eurasia continent in the recent half century, Geophysical Research Letters, 31, L12204, doi:10.1029/2004GL019814. Li Jian, Rucong Yu, Tianjun Zhou, and Bin Wang, 2005, Early Spring Cooling Trend Downstream of the Tibetan Plateau, Journal of Climate, in press [Rucong Yu]	
3-1565	A	55:48	55:50	The surface air temperature over the subtropical Eurasia continent, even eastward to the downstream of the Tibetan Plateau, are significantly correlated with NAO variability. Reference: Yu Rucong, Tianjun Zhou, 2004, Impacts of winter-NAO on March cooling trends over subtropical Eurasia continent in the recent half century, Geophysical Research Letters, 31, L12204, doi:10.1029/2004GL019814. Li Jian, Rucong Yu, Tianjun Zhou, and Bin Wang, 2005, Early Spring Cooling Trend Downstream of the Tibetan Plateau, Journal of Climate, in press [Tianjun ZHOU]	
3-1566	A	55:48	56:7	A very important review article concerning NAO which should be referenced is: Wanner H., et al., 2001: North Atlantic Oscillation - concepts and studies. Surveys Geoph., 22, 321-382. [Christian-D. Schoenwiese]	
3-1567	A	55:55	55:55	Replace Europe with "northern Europe". [Annarita Mariotti]	
3-1568	A	56:3	56:4	This reference to the NAO doesn't take into account its recent behavior. As is obvious from the CPC website data, (http://www.cpc.ncep.noaa.gov/products/precip/CWlink/pna/month_nao_index.shtml) 1968-1997 does not characterize recent data. Either drop the reference or recalculate it through 2004. [Jeffrey Kueter]	
3-1569	A	56:8	56:8	Here it could be mentioned that the NAO has a considerable influence on total ozone both through redistribution in the lower stratosphere accompanying the changing planetary	

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				wave structure and through a change in the planetary wave activity flux to the stratosphere, changing the residual circulation (Brönnimann, S., J. Luterbacher, C. Schmutz, H. Wanner and J. Staehelin (2000) Variability of total ozone at Arosa, Switzerland, since 1931 related to atmospheric circulation indices. Geophys. Res. Lett. 27, 2213-2216, Appenzeller, C., A. K. Weiss. and J. Staehelin (2000) North Atlantic Oscillation Modulates Total Ozone Winter Trends, Geophys. Res. Lett. 27, 1131-1134. Hartmann D. L., J. M. Wallace, V. Limpasuvan, D. W. J Thompson and J. R. Holton (2000) Can ozone depletion and greenhouse warming interact to produce rapid climate change? Proc. Natl. Acad. Sci. U.S.A. 97, 1412–1417.). [Stefan Brönnimann]	
3-1570	A	56:13	20:56	The upward trend toward more positive NAO index has led to a decreased height over East Asia associated with a tropospheric cooling. References: Yu Rucong, Bin Wang, and Tianjun Zhou, 2004, Tropospheric cooling and summer monsoon weakening trend over East Asia, Geophysical Research Letters, 31,L22212,doi:10.1029/2004GL021270; Xin XG, RC Yu, TJ Zhou, and B Wang, 2005, Droughty late spring of South China in recent decades, J. Climate, conditionally accepted; Yu Rucong, Tianjun Zhou, and Xiaoge Xin, 2005, Atmospheric cooling trend and the climate change over East Asia in the past 50 years, Journal of Climate, submitted [Rucong Yu]	
3-1571	A	56:13	20:56	The upward trend toward more positive NAO index has led to a decreased height over East Asia associated with a tropospheric cooling. References: Yu Rucong, Bin Wang, and Tianjun Zhou, 2004, Tropospheric cooling and summer monsoon weakening trend over East Asia, Geophysical Research Letters, 31,L22212,doi:10.1029/2004GL021270; Xin XG, RC Yu, TJ Zhou, and B Wang, 2005, Droughty late spring of South China in recent decades, J. Climate, conditionally accepted; Yu Rucong, Tianjun Zhou, and Xiaoge Xin, 2005, Atmospheric cooling trend and the climate change over East Asia in the past 50 years, Journal of Climate, submitted [Tianjun ZHOU]	
3-1572	A	56:13	56:20	This is a bit of a chichen-and-egg situation: when low-pressure systems pass over Iceland, they surely must affect the daily values of the NAOI, and a high frequency is likely to affect the monthly/seasonal values. Thus, a high NAOI may be just the consequence of more low-pressure systems over Iceland, not? [Rasmus E. Benestad]	
3-1573	A	56:13		The impact of the NAO on North Atlantic precipitation has been documented by Walsh and Portis, 1999; Bojariu and Reverdin, 2002; Mariotti and Arkin, 2005 (currentky being submitted). [Annarita Mariotti]	

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3-1574	A	56:19	56:20	Delete sentence commencing "Such changes…" detailing impacts (leave this for WG2) [Neville Nicholls]	
3-1575	A	56:22		The long-term trend of the NAO during winter has been associated with very important changes in the hydrological cycle in southern Europe with a significant increase in the fresh water deficit of the Mediterranean Sea over the past 50 yrs (Mariotti et al., 2002). [Annarita Mariotti]	
3-1576	A	56:22		River discharge over southern Europe was also significantly affected by the NAO (Struglia et al., 2002) [Annarita Mariotti]	
3-1577	A	56:34	56:43	Extrenal forcings such as tropical SST should play active roles in affecting the interannual variability of the SAM. Reference: Zhou Tianjun, Rucong Yu, 2004, Sea-surface temperature induced variability of the Southern Annular Mode in an atmospheric general circulation model?Geophysical Research Letters, 31,L24206,doi:10.1029/2004GL021473 [Rucong Yu]	
3-1578	A	56:34	56:43	Extrenal forcings such as SST should play active roles in affecting the variability of the SAM. When NCAR CAM2 model is forced by observational SST, it sucessfully reproduced the observational interannual variation of austral summer SAM. The source of much of the reproducibility is tropical Pacific SST, with equatorial Pacific warm event corresponds to a negative phase SAM in austral summer. Reference: Zhou Tianjun, Rucong Yu, 2004, Sea-surface temperature induced variability of the Southern Annular Mode in an atmospheric general circulation model?Geophysical Research Letters, 31,L24206,doi:10.1029/2004GL021473 [Tianjun ZHOU]	
3-1579	A	56:36		The zonal asymmetries (predominantly wavenumber 3) of SAM are of central importance to many aspects of regional SH climate variability. The oft-cited phrase ‘essentially a zonally-symmetric structure’ is perhaps misleading, and it would be nice to see a slight rewording. (See, e.g., Simmonds, I., and J. C. King, 2004: Global and hemispheric climate variations affecting the Southern Ocean. Antarctic Science, 16, 401-413.) [Ian Simmonds]	
3-1580	A	56:40	56:41	SAM isequivalent barotropic.. I do not understand [Bernard Seguin]	
3-1581	A	56:43		The recent work of Rashid casts considerable light on these matters and should be cited here: (Rashid, H. A., and I. Simmonds, 2004: Eddy-zonal flow interactions associated with the Southern Hemisphere annular mode: Results from NCEP-DOE reanalysis and a quasi-linear model. Journal of the Atmospheric Sciences, 61, 873-888. Rashid, H. A., and I. Simmonds, 2005: Southern Hemisphere annular mode variability and the role of optimal nonmodal growth. Journal of the Atmospheric Sciences, 62, 1947-1961.)	

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				[Ian Simmonds]	
3-1582	A	57:4		I would suggest deleting 'spatial variability in' here. [Nathan Gillett]	
3-1583	A	57:6	57:7	Again, I think you need to make clear that changes in the SAM during summer have been linked to the summer Peninsula warming, not the larger winter warming. [John Turner]	
3-1584	A	57:7	57:8	Reference is again made to the rejected Fogt and Bromwich (2005) GRL paper, although the information stated is accurate and refers back to Figure 3.5.1. The reference will need to be removed. [David / Ryan Bromwich / Fogt]	
3-1585	A	57:7	57:8	I suspect that the weakening of zonal winds in JJA is not significant. Also Figure 3.5.1 shows 200 hPa height, and therefore isn't relevant to near-surface winds over the Southern Ocean. According to Antarctic geopotential height trends shown by Thompson and Solomon (2002) over the past 30 years, JJA goepotenial height has indeed increased at 200 hPa as Bromwich and Fogt (2005) show, but it has decreased slightly near the surface. [Nathan Gillett]	
3-1586	A	57:25	57:25	Adding at the end of the line: Tropical SST influence on SAM variability on the interannual time scale are also demonstrated by ensemble simulations (Zhou and Yu, 2004). The corresponding reference should be added in the Reference List: Zhou T.J., and R.C. Yu, 2004: Sea-surface temperature induced variability of the Southern Annular Mode in an atmospheric general circulation model. Geophysical Research Letters, 31(24), L24206 [Dao-Yi Gong]	
3-1587	A	57:27	:35	Should the summary paragraph make a mention that both the SAM and the NAM show a strong increase, peak about at the same time and are flat/decreasing since? This is quite an amazing coincidence if it is a coincidence… [Gabriele Hegerl]	
3-1588	A	57:33	57:35	You must mean "attribution of the observed changes in SAM [in recent years] has been made to ozone depletion and increasing greenhouse gas increases." Because obviously the earlier changes identified by Jones and Widmann (2004) don't require an ozone or greenhouse gas explanation. [Jeffrey Kueter]	
3-1589	A	57:33	57:35	… increasing GHG increases (see Chapter 9), "and tropical ocean forcings (Zhou and Yu, 2004)". Reference: Zhou Tianjun, Rucong Yu, 2004, Sea-surface temperature induced	

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				variability of the Southern Annular Mode in an atmospheric general circulation model?Geophysical Research Letters, 31,L24206,doi:10.1029/2004GL021473 [Rucong Yu]	
3-1590	A	57:33	57:35	… increasing GHG increases (see Chapter 9), "and tropical ocean forcings (Zhou and Yu, 2004)". Reference: Zhou Tianjun, Rucong Yu, 2004, Sea-surface temperature induced variability of the Southern Annular Mode in an atmospheric general circulation model?Geophysical Research Letters, 31,L24206,doi:10.1029/2004GL021473 [Tianjun ZHOU]	
3-1591	A	57:33	:35	ADDITIONAL comment missing from other comment file: This statement sounds stronger to me than the one we make in chapter 9, lets cross check [Gabriele Hegerl]	
3-1592	A	57:34	57:34	It should be noted that Sexton et al included ozone forcing that was incorrect and led to too large a forcing in high latitudes. Therefore the reported strengthening is likely too strong in this paper. This has been shown to be the case if vertical temperatures are used as the tracer by comparing runs with corrected ozone with those using the incorrect ozone. [Peter Thorne]	
3-1593	A	57:35		insert "However, Fogt and Bromwich (2006) and L'Heureux and Thompson (2005) demonstrate relationships between the SAM and the tropical Pacific SSTs associated with ENSO events during austral spring and summer; thus the forcing the SAM is still uncertain." [David / Ryan Bromwich / Fogt]	
3-1594	A	57:44	58:14	A new observational and modelling paper has just appeared on natural variations in the thermohaline circulation: Knight, J., Allan R.J., Folland, C.K., Vellinga, M. and M.E. Mann, 2005: Natural Variations in the thermohaline circulation and future surface temperature. Geophys. Res. Lett., 32, L20708, doi: 1029/2005GL024233. [Chris Folland]	
3-1595	A	57:45	57:48	in text SST is in K and in Fig.3.6.8, in C [ILEANA MARES]	
3-1596	A	57:55	57:56	Figure 3.6.8 is called Atlantic Multidecadal Oscillation index but it appears to be virtually identical to Figure 3.2.6b where it is Northern Hemisphere warming. Is the AMO Index any different from rising SST in the Atlantic? I would have thought that the trend in temperature is associated with "global warming" rather than "the strength of the thermohaline circulation" (lines 55-56, page 3-57). [Harry Bryden]	
3-1597	A	57:56		A summary statement on changes in monsoon would be very helpful. [FILIPPO GIORGI]	

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3-1598	A	58:16	58:16	Figure 3.6.8 is not the clearest representation of the AMO. It is better presented in Goldenberg et al. 2001, Science, 293, 474-479; or Knight et al., 2005, GRL, 32, doi:10.1029/2005GL024233. [Jeffrey Kueter]	
3-1599	A	58:19	58:42	Remove discussion of the ACW. There is not concensus on whether this exists or how it is forced. [LuAnne Thompson]	
3-1600	A	58:26		The recent paper of White, W. B., and I. Simmonds, 2005: SST-induced cyclogenesis in the Antarctic Circumpolar Wave. Journal of Geophysical Research, (accepted) should be cited in this important connection of generating mechanisms. [Ian Simmonds]	
3-1601	A	58:40		References should be cited in chronological order. (This comment applies to many instances throughout the document.) [Ian Simmonds]	
3-1602	A	58:45	58:53	I cant see that you need this paragraph. It doesn't really summarise the interesting findings. [Neville Nicholls]	
3-1603	A	58:45	59:14	It would be nice to have at he end of the summary a sentence synthesizing on the existence, or not (or on the unavailability of sufficient data to…) of a trend of the main indeces. [Franco Desiato]	
3-1604	A	58:45		Section 3.6.7. The link between ENSO and Climate change is cited as a research question of great importance (cfr. p. 3-53 L 27-29). However, the remainder of section 3.6 and the summary given in section 3.6.7 does not really tackle that relevant question. The summary given in section 3.6.7 insists rightly upon the importance of natural oscillation patterns but does not conclude on the question raised. An additional comment on the possible link would be informative. [Philippe Tulkens]	
3-1605	A	58:47		Delete "low frequency". Variations in the frequency of El Nino can hardly be other than low frequency in the usual usage of the term. [Adrian Simmons]	
3-1606	A	59:0	64:	The section on monsoon ignores recent studies by Lohmann and Rotstayn (2002), Chung et al (J climate,2002), Menon et al (2002) and Ramanathan et al (PNAS,2005) which show substanatial sensitivty of the monsoon system to the north-south asymmetry in aerosol forcing. Lohman and Rotstayn and Ramanathan et al show that the summer time monsoon circulation weakens beacuse of wekaening of SST gradient (due to larger aerosol cooling of the NH oceans) and reduced evapoartion in response to the aerosol-	

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				induced dimming. There are numerous studies by Chinese, Japanese and Indian scientists on this issue. In stead the chpater relies more on GHGs modeling studies. [Veerabhadran Ramanathan]	
3-1607	A	59:9	59:10	As previous comment [John Turner]	
3-1608	A	59:13	59:14	Delete sentence starting "All of these.." This is about impacts. [Neville Nicholls]	
3-1609	A	59:16		I was surprised that no section was dedicated to Europe. Why? [Annarita Mariotti]	
3-1610	A	59:18	59:57	3.7.1. This is a nice overview of monsoons and their relation to other circulation "anomolies". [Jerry Mahlman]	
3-1611	A	59:20	61:11	A very useful additional diagrammatic figure would be one showing the global circulation belts such as Walker Circulation, Hadley and monsoon circulations etc and qualitative changes in these. I am not sure whether this is possible, given the information available, but this would be a useful synthesis tool. [M James Salinger]	
3-1612	A	59:28	59:28	The South west Pacific also have a summer rainy season, and a dry winter and would fit into this category of subtropical climates. [M James Salinger]	
3-1613	A	59:31	:33	Unclear what this sentence means. I could not find this reference (Trenberth et al., 2000) among the many Trenberth references! [Fons Baede]	
3-1614	A	59:31		Here's another reference to a Trenberth paper that can either be deleted or replaced by one of many other possible references. The statement is so general it hardly needs a reference. [Adrian Simmons]	
3-1615	A	59:36		Could "Maritime continent" be defined where first used, or possibly included in a glossary if there is to be one? I know it's used widely by tropical climatologists, but I don't think it's used by anyone else - it wasn't in the index of an Atlas, and a google search yielded only tropical meteorology web pages. [Nathan Gillett]	
3-1616	A	59:47		the major part of North Africa is not influenced by NH tropical monsoon regions, [Matari Amar]	
3-1617	A	59:47		The major part of North Africa is not influenced by NH tropical monsoon regions. [Amar Matari]	
3-1618	A	59:55	59:55	The rainfall trend in the SPCZ region described is not clear in figure 3.7.1 0 - rainfall has	

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				increased north east of the SPCZ, but decreased to the south west. [M James Salinger]	
3-1619	A	60:7	60:10	I think this paragraph could be deleted without losing anything. [Neville Nicholls]	
3-1620	A	60:12	60:17	Also see Ramanathan et al. (2005; PNAS, 102, 5326-5333). [Qiang Fu]	
3-1621	A	60:12	60:17	The mismatch of the stopped decline of monsoon in recent decade with the increase of pollutants rejects the idea of attribution of the declined monsoon winds to the increase in local aerosal-induced cooling. Recently, we have presented the observational evidences serving as a metric for validation of the climate model experiments that aim at explaining the causes of the "Yangtze River flooding and North China drought" trend pattern. For details, see: Yu Rucong, Tianjun Zhou, and Xiaoge Xin, 2005, Atmospheric cooling trend and the climate change over East Asia in the past 50 years, Journal of Climate, submitted [Rucong Yu]	
3-1622	A	60:12	60:17	Menon et al. (2002) view might be argued. As noted in the first comment, the mismatch of the stopped decline of monsoon in recent decade with the increase of pollutants rejects the idea of attribution of the declined monsoon winds to the increase in local aerosal-induced cooling.Explanation of the cause of the weakening of the summer monsoon has been extremely controversial. Numerical simulation would ultimately provide a convincing answer. However, given the wide range of the uncertainty in the realism of the current climate models, it is difficult to validate the various model results. Recently, we have presented the observational evidences serving as a metric for validation of the climate model experiments that aim at explaining the causes of the "Yangtze River flooding and North China drought" trend pattern. For details, see: Yu Rucong, Tianjun Zhou, and Xiaoge Xin, 2005, Atmospheric cooling trend and the climate change over East Asia in the past 50 years, Journal of Climate, submitted [Tianjun ZHOU]	
3-1623	A	60:12		Delete "even". The monsoons must have changed on timescales longer even than centuries. For sure due to continental drift. [Adrian Simmons]	
3-1624	A	60:15	60:17	Ramanathan et al. (2005, Proc. Natl. Acad. Sci.) is relevant here. [Nathan Gillett]	
3-1625	A	60:15	60:17	A little bit more detail would be welcome (in addition to Box 3.1): What is the effect of increased tropospheric aerosol loading on the monsoon system? Decreasing intensity? [Christian-D. Schoenwiese]	
3-1626	A	60:24	60:25	Delete this paragraph starting "The Asian monsoon has…".	

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				[Neville Nicholls]	
3-1627	A	60:27		One finds different trends in the monsoon depending upon which index is used. There has been a downward trend in wind-based monsoon indices yet almost no trend in all-India rainfall since 1950. See: Stephenson, D.B., H. Douville, and K. Rupa Kumar, 2001: Searching for a fingerprint of global warming in the Asian summer monsoon, Mausam (special issue), 52, 1, 213-220. [David Stephenson]	
3-1628	A	60:32		How is "representative" defined here? [Fons Baede]	
3-1629	A	60:33	60:33	Insert sentence: "Vinnikov and Robock (2002) showed that there has been no trend in either South Asian monsoon precipitation or its variability for the past century." ref: Vinnikov, Konstantin Y., and Alan Robock, 2002: Trends in moments of climatic indices. Geophys. Res. Lett., 29 (2), doi:10.1029/2001GL014025. [Alan Robock]	
3-1630	A	60:37	60:38	Southward shift of what? [Chris Folland]	
3-1631	A	60:37	60:51	A distinctive strong tropospheric cooling trend is found in East Asia during July and August. Accompanying this summer cooling the upper-level westerly jet stream over East Asia shifts southward and the East Asian summer monsoon weakens, which results in the tendency toward increased droughts in northern China and flood in Yangtze River Valley. References: Yu Rucong, Bin Wang, and Tianjun Zhou, 2004, Tropospheric cooling and summer monsoon weakening trend over East Asia, Geophysical Research Letters, 31,L22212,doi:10.1029/2004GL021270; Xin XG, RC Yu, TJ Zhou, and B Wang, 2005, Droughty late spring of South China in recent decades, J. Climate, conditionally accepted; Yu Rucong, Tianjun Zhou, and Xiaoge Xin, 2005, Atmospheric cooling trend and the climate change over East Asia in the past 50 years, Journal of Climate, submitted [Rucong Yu]	
3-1632	A	60:37	60:51	A distinctive strong tropospheric cooling trend is found in East Asia during July and August. Accompanying this summer cooling the upper-level westerly jet stream over East Asia shifts southward and the East Asian summer monsoon weakens, which results in the tendency toward increased droughts in northern China and flood in Yangtze River Valley. The observational evidences raise the possibility that the East Asian summer tropospheric cooling links to the stratosphere temperature changes and the interaction between the troposphere and stratosphere. Similar mechanism works in late spring. References: Yu Rucong, Bin Wang, and Tianjun Zhou, 2004, Tropospheric cooling and summer monsoon weakening trend over East Asia, Geophysical Research Letters,	

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				31,L22212,doi:10.1029/2004GL021270; Xin XG, RC Yu, TJ Zhou, and B Wang, 2005, Droughty late spring of South China in recent decades, J. Climate, conditionally accepted; Yu Rucong, Tianjun Zhou, and Xiaoge Xin, 2005, Atmospheric cooling trend and the climate change over East Asia in the past 50 years, Journal of Climate, submitted [Tianjun ZHOU]	
3-1634	A	60:41	60:41	"geopotential height over the northern Pacific (Gong et al., 2002)" should be changed to: "geopotential height and typhoon tracks in summertime over the northern Pacific (Gong et al., 2002, Ho et al., 2004) The new reference should be added in the Reference List: Ho C.-H., J.-J. Baik, J.-H. Kim, D.-Y. Gong, and C.-H. Sui., 2004: Interdecadal changes in summertime typhoon tracks. Journal of Climate, 17, 1767-1776 [Dao-Yi Gong]	
3-1635	A	60:53	61:11	This paragraph reads too much like a list without a coherent story. [Chris Folland]	
3-1636	A	60:53	61:11	The tropopsheric biennail oscillation (TBO; Meehl (1987, MWR) is another important variability of the Indain monsoon and is worthy of mentioing. The phenomenon involves interactions among the monsoon, Indo-Pacific Oceans, and EuroAsia continent.The interaction and relationship between ENSO and TBO is still not clear and has been an area of active research. [Jin-Yi Yu]	
3-1637	A	60:53		You use "South Asian" in line 25, and "indian" here, for the same phenomenon (I think). [Neville Nicholls]	
3-1638	A	61:6		Are the large-scale changes related to the weakening of the relationship? Ambiguous. [Neville Nicholls]	
3-1639	A	61:17	61:18	Please explain briefly MJO mechanism. [Christian-D. Schoenwiese]	
3-1640	A	61:19	61:20	So what? [Chris Folland]	
3-1641	A	61:19	61:20	I suggest moving the sentence "Trends in CAPE in northern Australia are weak and not significant (Gettelman et al., 2002; DeMott and Randall, 2004)" to the end of the paragraph. That is, move it to line 30. [Matthew C. Wheeler]	
3-1642	A	61:25	61:25	2003/04 should be changed to 2004/05 (note that the data in this figure was updated between the zeroth and first-order drafts to include the latest monsoon season). [Matthew C. Wheeler]	
3-1643	A	61:32	61:38	But, again we are stuck with the "trend" attribution problem. Is it made intractable when	

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				we pursue "trends of extreme events? Is this an oxymoron when we factor in all of the messy and incomplete data?. Some of my statistician friends lean toward suspecting that it is, at least on the time scales that strongly test our patience to unscramble all of this as the world awaits "definitive answers". [Jerry Mahlman]	
3-1644	A	61:34	61:34	Where in the Pacific? [Chris Folland]	
3-1645	A	61:43	61:56	There is nothing on multidecadal drought periods, perhaps because its in Chapter 5. Cross refer if so. [Chris Folland]	
3-1646	A	61:43	62:10	There should be a figure here to document the changes mentioned in the writeup. [Henry Diaz]	
3-1647	A	62:1	62:10	Section 3.7.3. This vatriability is also observed in Amazonia. The paper does not suggest a southward shift of the SAMS, [Jose Marengo]	
3-1648	A	62:21	62:25	Over what period is the correlation calculated. Is it significant given likely strong persistence? [Chris Folland]	
3-1649	A	62:27	62:39	To be fair, the Sahel drought has been disputed but not by many. A problem with Fig 3.7.5 and many diagrams in this Chapter is a lack of error estimates - that particularly allow for the often large time-varying data gaps. This problem might be noted more generally somewhere in this chapter, perhaps in conclusions, and the need to assess uncertainties (not completely straightforward when climate is changing). [Chris Folland]	
3-1650	A	62:32	62:34	If 'climate change' means 'anthropogenic climate change', which in IPCC language, I think it does, then I think this is much too strong. Some researchers have argued that the changes in Sahel rainfall are just natural variability (e.g. Nicholson, 2001), therefore I think 'undisputed' is not correct. [Nathan Gillett]	
3-1651	A	62:34	62:34	change 'most undisputed' to 'least disputed' [Rasmus E. Benestad]	
3-1652	A	62:36	62:36	Reference is made to the "major El Niño event of 1983-84" This El Niño event actually covers the years 1982-1983. [Henry Diaz]	
3-1653	A	62:36	62:36	The El Niño event was in 1982/83, not in 1983/84. [Martin Stendel]	

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3-1654	A	62:51	62:55	What about the well known quasi-bidecadal variability of South African rainfall? Has it gone way? Fairly recently, Folland et al (1999) (in the Chapter ref. list) described a large scale SST pattern associated with this phenomenon (their EOF4). [Chris Folland]	
3-1655	A	62:55		should there be a summary paragraph for tropical rainfall changes? [Gabriele Hegerl]	
3-1656	A	63:2		Section 3.7.2 Notwithstanding the title of this section, the Walker Circulation is hardly mentioned. I suggest to add a paragraph on the Walker Circ. at line 17, parallelling the para on the Hadley Circ, lines 4 to 16 [Fons Baede]	
3-1657	A	63:4	64:4	Reference should be made to chapter by Quan et al. (2004) in book volume: The Hadley Circulation: Present, Past and Future, edited by H.F. Diaz and R.S. Bradley, Kluwer Academic Publs. [Henry Diaz]	
3-1659	A	63:18	63:19	I disagreed with a few statements on topics that I am familiar with. 2. The statement on lines 18-19, page 3-63 "From the Tropics to about 31 latitude, the primary poleward energy transport mechanism is the Hadley and Walker overturning circulation" is in conflict with Trenberth and Caron's figure 7 that shows ocean heat transport dominates the atmospheric heat transport from about 10 S to 20 N. [Harry Bryden]	
3-1660	A	63:18	63:19	Doesn't the Walker Circulation just transport heat in the zonal direction, with no contribution to poleward heat transport? [Nathan Gillett]	
3-1661	A	63:47	63:47	Insert sentence: "Vinnikov and Robock (2002) showed that while there has been a slight downward trend in the SOI (more El Niños) for the past century, there has been no trend in its variability." ref: Vinnikov, Konstantin Y., and Alan Robock, 2002: Trends in moments of climatic indices. Geophys. Res. Lett., 29 (2), doi:10.1029/2001GL014025. [Alan Robock]	
3-1662	A	63:53	63:53	I suggest to introduce a recent reference : Frankignoul C. and E. Kestenare, 2005: Air-Sea Interactions in the tropical Atlantic: A view based on lagged rotated maximum covariance analysis.J. Climate, 18, 3874-3890 [ILEANA MARES]	
3-1663	A	64:2	64:4	See last comment - this is where a summary circulation diagram showing changes would be useful. [M James Salinger]	

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3-1664	A	64:6	64:47	Worth mentioning somewhere that on monthly and seasonal average time scales the following paper has analysed changes in surface temperature extremes over both global land and oceans using the variance corrected HadCRUTv data set: Horton, E.B., Folland, C.K. and D.E. Parker, 2001: The changing incidence of extremes in worldwide and Central England temperatures to the end of the twentieth century. Climatic Change, 50, 267-295. [Chris Folland]	
3-1665	A	64:6	77:16	Maybe I was getting tired, but Section 3.8 was written in a complex style, different from earlier sections, that obscured the results for me. As an example, I suggest reading the summary for Precipitation on lines 33-35 on page 3-68 and explaining what the conclusion is. I certainly did not take away from 3.8.2.2 the succinct thoughts on Precipitation contained in the Synthesis, lines 15-18 on page 3-80. I had a similar experiences with all of Section 3.8: I would have been better informed if I just read the Synthesis statements. [Harry Bryden]	
3-1666	A	64:6		Section 3.8, particularly section beginning line 47: There should be more discussion of available datasets. References to some of these regional datasets would be useful for other researchers. [John Caesar]	
3-1667	A	64:6		Section 3.8: particularly section beginning line 47: The section says "Results are now available from newly established regional- and continental-scale daily datasets." We have results available from a newly established near-global daily dataset (Caesar et al., 2005, JGR-Atmos, in press) of gridded daily maximum and minimum temperatures. This dataset is currently referenced in Chapter 9, so it would seem inconsistent not to include it in the observational chapter. [John Caesar]	
3-1668	A	64:10	64:11	A more usual and appropriate definition is that extreme events or extreme data, respectively, are relatively far from the average or median, say two or three times of the standard deviation (only appropriate in case of Gaussian distributed data) or above/below upper/lower percentiles (e.g. 5% or 10%, 95 % or 90%). Wether such extremes are rare or not or wether their probability of occurrence may change to be no longer rare depends on the related probability function (PDF) and its change in time. [Christian-D. Schoenwiese]	
3-1669	A	64:12	64:12	There is a new method available which allows to compute exactly the time history of all PDF parameters (e.g. for every year of observation) and, in turn, the change of probabilty that upper or lower extremes (see comment 27) may occur; see list of references, p. 108, lines 13-16. The same authors (Trömel and Schönwiese) have also computed, based on	

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				this method, the probability change of occurring upper and lower extremes (percentiles) of precipitation in Germany where it arises that winter precipitation PDFs tend to "broaden" (both more upper and lower extremes), in contrast to summer precipitation. Reference: Trömel S. and C.-D. Schönwiese, 2005: Probability change of extreme precipitation observed 1901-2000 in Germany. Theor. Appl. Climatol., revised version submitted. Results for Europe are published only in German, so far. [Christian-D. Schoenwiese]	
3-1670	A	64:14	64:15	For any change in the mean, there will always be larger changes in extremes of one or both signs on a percentage basis." I don't think that this statement is valid in this general form. I suggest that this sentence is omitted or (if the purpose is to allude to the sensitivity of extremes to changes in the mean) replaced by something like: "Changes in the mean can be associated with large relative changes in the frequency of extremes. [Christoph Frei]	
3-1671	A	64:14	64:15	"For any change in the mean, there will always be larger changes in the extremes of one or both signs on a percentage basis". The authors need to say larger than what i.e. larger changes in probability of occurrence of extremes compared to the change in probability of occurrence of more moderate events. [Nathan Gillett]	
3-1672	A	64:14	64:15	I believe the statement "For any change …percentage basis" is not universally true, and depends on how the distribution of events changes. [FILIPPO GIORGI]	
3-1673	A	64:14	64:15	The statement "For any change in the mean, there will always be larger changes in extremes of one or both signs on a percentage basis" simply cannot be true for all climate variables. It is not at all impossible to simply increase frequency of the mean and shorten the tails, for example. Delete statement. Compare this with lines 42-44, page 65: "…observed changes of the tails of the temperature distributions are often more complicated than a simple shift of the entire distribution would suggest". [Jeffrey Kueter]	
3-1674	A	64:14	64:15	This statement is dubious. How do you compare changes in mean and extremes on a percentage basis? And why couldn't a distribution simply shift with no change in shape? [Steven Sherwood]	
3-1675	A	64:15	64:17	Delete this impacts statement. [Neville Nicholls]	
3-1676	A	64:15	64:15	Add the following short paragraph here: Since the TAR, Vinnikov and Robock (2002) presented a fundamentally new technique for analyzing trends in variance and higher order moments of climatic time series. They illustrated the technique by showing that	

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				there have been no trends in the past 100 years in the variance of US precipitation, US Palmer Drought Index, or All-India Monsoon rainfall. ref: Vinnikov, Konstantin Y., and Alan Robock, 2002: Trends in moments of climatic indices. Geophys. Res. Lett., 29 (2), doi:10.1029/2001GL014025. [Alan Robock]	
3-1677	A	64:20	64:21	"for climate extremes we need to look for a pattern of behaviour over several synoptic weather events". I'm not sure about this statement. It may need to be much more than several weather events that one has to look over to see what is going on. And some may view the occurrence of a single record weather event as an extreme of climate. Mumbai has a monsoon climate characterised by heavy rainfall events, and the heaviest recorded of these (what might be called the climate extreme) occured on a single day in July this year. [Adrian Simmons]	
3-1678	A	64:24	64:24	Also in the TAR,..' would be better as "As in the TAR,…" [John Caesar]	
3-1679	A	64:26	64:27	See again comment 28. [Christian-D. Schoenwiese]	
3-1680	A	64:30	64:31	Not only days but also months or seasons may be extreme, see e.g. Boxes 3.5.4 and 3.5.5, in addition, even years or a couple of years. [Christian-D. Schoenwiese]	
3-1681	A	64:51	64:51	The role of common definition of indices should be evocated : ECA or STARDEX project, Frich or Easterling lists. It facilitates exchanges, comparisons, analysis [Jean-Marc Moisselin]	
3-1682	A	65:0		On a more positive note. In the midst of considerable "trends of extremes" opacity of understanding, and the inadequacy of the most interesting of such regional "trends", there are a number of regional-scale measurement programmes that may provide a considerably distilled guidance on how we might address this problem in a reasonably coherent way. To me, some of the examples here show encouraging rigour, while other appear to be statistical nightmares. I thus suggest that a genuine effort be made to extract the most robust physical and statistical information from the mega-piles of regional data that are still underanlyzed, and setting aside those too opaque, too short, and too contaminated data sets that only add confusion to the already difficult quest. Choosing "clean" data sets that have true trend coherence for careful analysis could yield important new insights on regional manifestations of the emerging global-warming signals. With those discipline applied, perhaps "trends of extremes" might no longer be an oxymoron. [Jerry Mahlman]	
3-1683	A	65:1		Delete "for the era" as the word era appears later in the line.	

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				[Adrian Simmons]	
3-1684	A	65:4		Add "improved" before "satellite", as VTPR data (and PAOBs from imagery) were in use before 1979. [Adrian Simmons]	
3-1685	A	65:4		Change "storm activity" to "analysed storm activity over the southern oceans". Bengtsson et al did report a change in the global value in 1979, but it almost certainly came primarily from the southern hemisphere. It is here that better satellite information (and drifting buoys) would be expected to really change estimates of storm activity from 1979 onwards. Over the northern hemisphere we have the weather ships and much more VOS data giving coverage in the pre-1979 period. We should not undersell what we can say regionally (see also comment 12) even if we do not have a good global picture. After all, one of the things we want to use observations or reanalyses to do is to verify the past representation of the atmosphere in the global models that are used to predict future climate. Even if we cannot verify their performance globally, we can at least check it out for some regions. If we know how the atmosphere has changed for some regions, but not the globe, we have "necessary but not sufficient" tests for our models. [Adrian Simmons]	
3-1686	A	65:13		One approach to assess changes in extreme events is to estimate and predict (extreme) quantiles. The extreme quantiles tell us about the changes in the extreme events or in the tails of the underlying probability distribution function (cf. Ghosh & Draghicescu 2002 a,b). On the other hand, in the presence of heavy events, such as for wind-speed data, extremely high wind-speeds may act as influential observations. In such a case, rather than extreme quantiles, the median can bring out other information. For instance, Beran et al. (2003) demonstrate that for data with very large or very small observations (outliers) such as wind-speed data with storm events, a robust estimation approach (e.g. local M-estimation fit) leads to less adverse effect of high wind-speeds on estimation. Due to the extreme values, the mean curve tends to stay above the median during mid-winter, coinciding only in the summer - exhibiting seasonality in the distributional pattern of storms. [Sucharita Ghosh]	
3-1687	A	65:15	65:15	Please, add short information about analysis that found no trends in climate variability: Vinnikov and Robock (2002) have analyzed trends in moments of a few large scale climatic indices to determine whether the observed climate is getting more or less variable. The observed 1901-2000 New York City sea level variations, U.S. annual average precipitation, U.S. annual averages of the Modified Palmer Drought Severity Index, All-India Monsoon Rainfall Index, and Southern Oscillation Index have been	

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				studied. The trend showed that the observed 20th Century global warming trend has been accompanied by trends in the first statistical moments of some, but not all, climatic indices. Contrary to their expectation, no significant trends in the second, third, and fourth moments of the statistical distributions of five selected climatic indices have been found. This means that there is no statistically recognizable trend in variability of any of these climatic indices for the past 100 years. [Konstantin Vinnikov]	
3-1688	A	65:16	66:66	Comment: There has been some analysis on number or record-breaking events: Benestad, R.E. (2003) How often can we expect a record-event? Climate Research Vol 23, 3-13; Benestad, R.E. (2004) Record-values, non-stationarity tests and extreme value distributions Global and Planetary Change vol 44, issue 1-4, p.11-26. There may also be other publications on record-events, but this subject is not discussed in AR4 chapter 3. For instance, Benestad (2004) found the number of record-breaking monthly mean temperatures world wide to be stginficantly higher (outside the 90% confidence interval) than expected from an iid process, whereas Benestad (2003) found anomalously higfh number of record-events in monthly maximum temperaturs in Fennoscandia. [Rasmus E. Benestad]	
3-1689	A	65:17	66:48	Frost may be an extreme event in Florida, but it is not in many parts of the world. The stuff on frost days, warm nights, etc. in 3.8.2.1 repeats what is spelt out very well already in section 3.2. Section 3.8.2.1 should be restricted to real extremes. [Adrian Simmons]	
3-1690	A	65:17		Section 3.8.2.1: There is some lack of clarity in this section through the mixing-up of studies based on actual temperature data, and those based upon indices data. The reference to Robeson (2004) may be better placed further on at page 66 from line 2, as the study deals with mutual changes to the warm and cold tails of the distribution (c.f. Moberg and Jones, 2005). [John Caesar]	
3-1691	A	65:24	65:37	Section 3.8.2.1: Robeson (2004) has found intense warming of the lowest daily minimum temperatures over North America, though it should be noted that this was for the boreal winter DJFM). Caesar et al. (2005, JGR-Atmos, in press) have confirmed these results over the USA and found similar (and indeed more intense) patterns of change in the lowest daily minimum temperatures over Europe, China and Russia. [John Caesar]	
3-1692	A	65:29	65:30	The results of Robeson (2004) are mischaracterized. Quoting directly from Robeson (2004): “Using these methods, intense warming is found in the lowest minimum temperatures over western and central North America. During the months of January through March, the lower tail of the daily minimum air temperature distribution over	

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				western North America has warmed at rates exceeding 3ºC/50yr…Other times of year in western North America, as well as much of eastern North America, show little change in either minimum or maximum air temperature during the last half-century." This is a far cry from lines 29-30 which state "…as well as intense warming of the lowest daily minimum temperatures over North America (Robeson, 2004)." Further, this section completely ignores the results of Robeson (2003—Climate Research, 22, 205-213) that in the United States, there is an inverse or weak relationship between the mean and the standard deviation of daily air temperature and that implies that that interdiurnal variability of air temperature should either decrease or remain unchanged under warming conditions. And, the section on temperature extremes ignores the results of Knappenberger et al. (2001, Climate Research, 17, 45-53) who find that in the United States, from 1970-1997, the predominance of the warming has occurred during the coldest days and coldest nights of the year, while the temperatures during warmest days and warmest nights have changed relatively less. This is opposite in sense to the warming the occurred from 1910-1939 which was manifest more strongly as a warming of the warmest days. This result supports the result of Robeson (2002) which indicates that the recent increase in mean temperatures has been associated with declining temperature variability. [Jeffrey Kueter]	
3-1693	A	65:39	65:50	Section 3.8.2.1 is for southern and central South America [Jose Marengo]	
3-1694	A	65:39		Note that in a recent study (Fowler, H.J. and Archer, D.R. Conflicting signals of climatic change in the Upper Indus Basin. Journal of Climate, accepted subject to minor revisions) we found that daily temperature range is increasing in the Karakoram region of the Himalaya - despite trends to the contrary over most of the rest of the globe. We also found winter warming (since 1900) but summer cooling (due to a cooling of both maximum and minimum temperatures in summer). We have found no good explanation for this so far but cloudiness may provide a solution. [Hayley Fowler]	
3-1695	A	65:46		This subchapter provides an excellent discussion on evidence for changes in extremes, such as TN10 indices and others. However a discussion on evidence of changes in measures of observed inter-annual variability (such as standard deviations of seasonally averaged quantity) is missing. The latter is a key issue later on in the AR4 report (see e.g. the chapter on regional climate modelling, chapter 11 page 36 line 45) . There are not many studies on changes in the distribution of observed seasonal surface temperature and hence the available one should be included. Scherrer et al. investigated	

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				standardized distribution changes in central Europe using observations between 1961 and 2004 and found temperature variability changes in Europe show a weak increase (decrease) in summer (winter) for the time period 1961 to 2004, but these changes are not statistically significant at the 90% level. Reference: Scherrer, S.C, C. Appenzeller, M. A. Liniger and C. Schär, 2005: European temperature distribution changes in observations and climate change scenarios. GEOPHYSICAL RESEARCH LETTERS, VOL. 32, L19705, doi:10.1029/2005GL024108. [Christof Appenzeller]	
3-1696	A	66:0		Table 3.6 : difficult to understand : percentage of percentage for the evolution. Why do not use the classical name of such indice tn10p, Tn90p,…. Choice of Upper and Lower decile value could be better. [Jean-Marc Moisselin]	
3-1697	A	66:10		Beniston and Stephenson (2004) used a quantile approach to show that changes in extremes of daily temperature in Switzerland were due to changes in both the mean and the variance of the daily temperatures: Beniston, M., and D.B. Stephenson, 2004: Extreme climatic events and their evolution under changing climatic conditions, Global and Planetary Change, 44, 1-9. The fact that extremes such as heat waves are due to changes in variance as well as the mean is an important climate change result that needs to be mentioned in this chapter. [David Stephenson]	
3-1698	A	66:14		After "Mediterranean coast.", add: "In the swiss Alps, Rebetez (2004) shows that the unusual nature of the 2003 heat wave was not so much the absolute daily extreme values as the lack of cool temperatures and the large number of very warm days." [Rebetez, M, 2004: Summer 2003 maximum and minimum daily temperatures over a 3300 m altitudinal range in the Alps. Clim. Res. 27: 45-50] [Martine Rebetez]	
3-1699	A	66:23	66:23	Should read 'took' not 'tok'. [John Caesar]	
3-1700	A	66:23	66:23	Change "tok" to "took". [Qiang Fu]	
3-1701	A	66:24	66:24	Spelling of 'took'. [M James Salinger]	
3-1702	A	66:27	66:29	The text states that Figure 3.8.1 shows that "the distribution of minimum temperature and that of maximum temperature have not only shifted, but also changed shape." However, the figure refers to cold minimum temperatures and warm minimum temperatures. Also,	

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				the distribution of the index referred to may have changed over time, but this is not the same as saying that the whole maximum or minimum temperature distribution has changed, which is what the text implies. [John Caesar]	
3-1703	A	66:27	66:29	Change is strong for 1979-2003, not for 1951-2003 [Jean-Marc Moisselin]	
3-1704	A	66:29	66:32	Please, check this sentence. [Michele BRUNETTI]	
3-1705	A	66:29	66:32	The sentence refers to the change in the tails of the distributions, as was very well illustrated by the Figure 2.32 on page 155 of the TAR. Will a similar diagram be included, or at least referenced, in this report, as it is very good for explaining to the issues with changing extremes to those less familiar with the subject? [John Caesar]	
3-1706	A	66:29	66:32	I do not understand the sentence, if the change is the same, the variability is unchanged. [Jean-Marc Moisselin]	
3-1707	A	66:29	66:32	Sentence not clear. [Teresa NANNI]	
3-1708	A	66:29		Figure 3.8.1 does not show distributions of minimum and maximum temperature, or anything that can directly be related to these as far as I can tell. If 3.8.1 shows a distribution over separate years, then changes in the shape of the distribution tell us about changes in interannual variability, not changes in the distribution of minima and maxima. [Nathan Gillett]	
3-1709	A	66:34	66:38	Must specify what period the 10th and 90th percentiles represent - years 1961-90? [Rasmus E. Benestad]	
3-1710	A	66:34		Table 3.6 is hard to understand. [Qiang Fu]	
3-1711	A	66:35	66:35	Don't need to mention the italics in the legend, since the table has no italicized values. [Dian Seidel]	
3-1712	A	66:37	66:38	".. without estimates of intrinsic uncertainties .." (occurs in several Table legends). What is meant by intrinsic uncertainty? Maybe it would be more appropriat to state what types of error are contained in the confidence intervals. [Christoph Frei]	
3-1713	A	66:47	66:47	Captions of Figs 3.8.1 and 3.8.2 should refer to Alexander et al (2005a) not 2005. [Chris Folland]	
3-1715	A	67:1		There is an evidence of increasing heavy rainfall and decreasing low rainfall in a hundred year precipitation record in Japan. Fujibe et al. (2005) found that the linear trend for the	

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				heavy rainfall is • +20-30% per century, while that for low rainfall is –20-30% per century. This feature is found invariably for four-hourly, daily, and hourly precipitation, and qualitatively for all the seasons and regions in Japan. JMA (2005) show that the same is true over broad regions in East Asia. Fujibe, F., N. Yamazaki, M. Katsuyama, and K. Kobayashi, 2005: The increasing trend of intense precipitation in Japan based on four-hourly data for a hundred years. SOLA, 1, 41-44, doi:10.2151/sola.2005-012. Japan Meteorological Agency (JMA), 2005: Ijyou Kisyou Report 2005 (Report on Recent Climate Change 2005 –Reviews and Outlook for the Future–). 374 pp. (in Japanese) (available on-line from http://www.data.kishou.go.jp/climate/cpdinfo/climate_change/) [Akio Kitoh]	
3-1716	A	67:2	67:3	This sentence is awkward. Please rewrite it. [FILIPPO GIORGI]	
3-1717	A	67:4	67:4	The reason of the statement "but reduced frequency or duration" should be explained more clearly [Franco Desiato]	
3-1718	A	67:4	67:5	Why does warming necessarily imply 'reduced frequency or duration' of precipitation? [Nathan Gillett]	
3-1719	A	67:7	67:7	TAR uses 50mm/day and the upper 5 percentiles as “heavy and extreme precipitation”. “Heavy” may apply, but “extreme” surely does not apply to one-in-twenty rain events, and it should be dropped. [Jeffrey Kueter]	
3-1720	A	67:10	67:24	Concerning European trends in extreme precipitation. Besides estimates of the growing extreme precipitation for the last 50 years, it can be mentioned that Zolina et al. (2005) analysed centennial-scale trends in extreme precipitation from the daily rain gauge data using different indices and found that estimates of annual indices derived for the locations where different indices shows significant trends imply primarily positive centennial-scale changes in heavy and very heavy precipitation with the strongest magnitudes of about 3–5% per decade in the Northern European Russia. Reference: Zolina, O., C. Simmer, A. Kapala, and S.K. Gulev, 2005: On the robustness of the estimates of centennial-scale variability in heavy precipitation from station data over Europe. Geophys. Res. Lett., 32, doi:10.1029/2005GL023231. [Olga Zolina]	
3-1721	A	67:14	67:14	Insert sentence: "Vinnikov and Robock (2002) showed that while there was a significant	

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				upward trend in US mean annual precipitation for the past century, there has been no trend in precipitation variability." ref: Vinnikov, Konstantin Y., and Alan Robock, 2002: Trends in moments of climatic indices. Geophys. Res. Lett., 29 (2), doi:10.1029/2001GL014025. [Alan Robock]	
3-1722	A	67:20	67:23	This statement about greater precipitation increases in the extremes when compared with the mean in the United States is not supported by work by Michaels et al. (2004, Int. J. Climatology, 24, 1873-1882), who conclude that: Our results support the contention that, where changes are significant, there is an increase in the amount of rain occurring on heavy rain-days. However, our results provide no support for the contention that the increase in total annual rainfall observed across the United States is disproportionately occurring on the wettest days—a contention that may have arisen from methodological constraints rather than true changes in the nature of precipitation delivery. After allowing for the total rain increases within each of our seven regions, we find no consistent national behavior in the U.S. precipitation record. Increases are indeed disproportionate for ranked days four through ten in the Southeast, but there is a balancing disproportionate decrease in the Northwest and in the Pacific Southwest. Our results argue strongly that the increase in rainfall in the coterminous 48 states that has been observed in the last 100 years has not resulted in any systematic disproportion in the percent of that increase allocated to the heaviest rain days. At the very least, this conflicting finding should be mentioned, considering that Michaels et al. (2004, Int. J. Climatology, 24, 1873-1882) set out specifically to determine whether the extremes were changing at a different rate than the mean—an issue that is being directly discussed in this section of Chapter 3. [Jeffrey Kueter]	
3-1723	A	67:41	67:41	"the change has lead to a weakening of the continental trough" Do you mean the precipitation change or some other change? This sentence is unclear. [Dian Seidel]	
3-1724	A	67:41		The change in precip has led to a weakening of the trough? Surely this is the wrong way round. [Nathan Gillett]	
3-1725	A	68:0	73:0	This hurricane-oriented discussion highlights a good example of a quite-erratic time series that, nevertheless, appears to have reasonably robust "trends of intenstiy/variablity statistics" that recently have exposed some very important hurricane intensity increase statistics, well in line with model-based projections a decade ago(which, of course, are not	

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				referencable in AR4!) [Jerry Mahlman]	
3-1726	A	68:0		Environmental factors such as generation of synoptic-scale cyclonic disturbances/waves and wind shear are extremely important in TC formation—these factors are very likely changing as general circulation changes and these changes should be mentioned. The ENSO effect is mentioned on p. 3-70 and 3-71, but should also appear briefly in the summary Box 3.4. [Richard Anthes]	
3-1727	A	68:6	68:6	Should read 'insight into' not 'insight in'. [John Caesar]	
3-1728	A	68:6	68:18	This paragraph seems to suggest that there are long-term (century-scale) variations in precipitation extremes. This seems to contradict earlier discussion of trends in precip extremes in recent decades. These views need to be reconciled. [Dian Seidel]	
3-1729	A	68:6	68:18	Suggest add the following (or the like) in this paragraph (maybe at the end of it): "Wang (2005) and Hanesiak and Wang (2005) find significant increases in the frequency of freezing precipitation occurrence in northern Canada (north of 50N), with decreases in southern British Columbia, central Prairies, and the Great Lakes Area in autumn and winter." [Xiaolan L. WANG]	
3-1730	A	68:9	68:10	At least in Germany the summer 2002 was very wet only within a relatively small subregion; at the same time other regions were rather dry. Such regioinal and subregional peculiarities are typical for the mid-latitude continental summer (small-scale convection). [Christian-D. Schoenwiese]	
3-1731	A	68:14	68:14	Change “frequency of precipitation “ with “frequency of extreme precipitation”. [Aristita Busuioc]	
3-1732	A	68:14	68:57	Re: tropical cyclone severity. The associated figure 3.8.3. implies significant problems with highly-cited Webster and Emanuel papers on hurricane intensity. Records for the North Atlantic and the Western North Pacific are very good back to 1948. Very few things got missed in the western Pacific because there are so many islands, and we were playing tag with the Commies all over the place out there. In the eastern North Pacific, prior to satellites we simply were unaware that there were so many tropical cyclones out there. In actuality, there’s no real evidence for sudden rise around 1970, when the satellite record begins. Adding all of the decent data from 1948 on (meaning Atlantic and Western Pacific) and combining it with the chart beginning in 1970, shows no significant increase in ACE over the period of record. Further, you will see that the available ACE data (which just happens to cover the world’s two most active basins) from fifty years ago	

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				are undistinguishable from the current era. The text must be modified to reflect this. In other words, a meta-analysis which uses the available reliable data certainly undercuts much of what is out there in the press today. [Jeffrey Kueter]	
3-1733	A	68:20	68:20	Figure 3.8.2. Michaels et al. (2004, Int. J. Climatology, 24, 1873-1882) clearly demonstrated that the methodology employed by Groisman is insufficient to distinguish whether there has been a disproportionate increase in the upper percentiles as compared to the mean, and yet Figure 3.8.2 presents the results using such methodology. Michaels et al (2004) simulated the methodology of Groisman using randomly generated precipitation changes with known attributes and concluded: In each case, we simulate a change in precipitation that is not disproportionately biased to the extreme events, and yet in each case, the results from using a fixed-bin approach appear to indicate that the increase has been primarily manifest in the highest bins (i.e. the ones containing the most extreme events). This clearly demonstrates the weaknesses and limitations of a fixed bin approach in assessing precipitation changes—the true nature of the underlying change is obscured by the analysis. Therefore, conclusive statements about the proportionality (or disproportionality) of the observed changes cannot be reliably made. Again, we are not suggesting that the fixed-bin approaches are not sensitive to changes in the underlying distribution of daily precipitation events that are truly disproportionate across bins, but simply that these techniques cannot differentiate between proportionate and disproportionate precipitation changes in all cases. To accurately assess the proportionality of observed changes, a different analysis technique must be employed. Thus, the information contained in Figure 3.8.2 cannot be interpreted in meaningful fashion. [Jeffrey Kueter]	
3-1734	A	68:22		does "higher" mean longer? [Steven Sherwood]	
3-1735	A	68:35		Is there a comparison somewhere of how annual total rainfall changes vs how extreme rainfall changes, on averages and regionally? I might have missed it… it would be nice to have such a comparison. [Gabriele Hegerl]	
3-1736	A	68:39		Box 3.4: In view of this year's very active tropical cyclone season, this box is likely to be read by non-experts. In its present form it is unsuited to that purpose. I suggest to rewrite it, avoiding such terms as CAPE and other technical terms. [Fons Baede]	
3-1737	A	68:41		The attribution of warming to human inducement is unnecessary	

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				and in appropriate for this chapter. Further, this box seems to be focused on predicting the future rather than indicating what trends to look for in the historical record, the subject of this chapter (i.e., "SSTs are expected to rise" should be "SSTs have increased") The rest of this box is largely incoherent. CAPE is not explained to the reader. Moreover, there is no evidence that CAPE per se has anything to do with tropical cyclones. The potential intensity of hurricanes (Emanuel's theory) is not related to CAPE, which in his theory is assumed negligible, but to surface and subsurface temperatures. The frequency of occurrence of hurricanes is also probably determined mainly by the nature of the wind field and by nonlocal forcing mechanisms like the African disturbances, with sufficient CAPE a necessary but far from sufficient condition. Since there are no clear trends in CAPE anyway, any relation of storm characteristics to CAPE---even if there were one---would be irrelevant to a discussion of storm changes over the 20th century. [Steven Sherwood]	
3-1738	A	68:41		Change "As human-induced climate change increases global warming" to "As sea surface temperatures rise," The words "increases global warming" suggest there would be global warming even without human-induced climate change. Maybe, but this chapter is not the place to go into that. [Adrian Simmons]	
3-1739	A	68:45	68:46	Can you write this without mentioning CAPE? Difficult concept for average reader. [Neville Nicholls]	
3-1740	A	68:48	68:48	Emanuel (2003) is a more appropriate reference than Emanuel (2005). [Matthew C. Wheeler]	
3-1741	A	68:49	68:49	Change "increase radiative heating" to "decrease radiative cooling". [Qiang Fu]	
3-1742	A	68:53	68:54	CAPE is a computed from the vertical profiles of temperature and moisture. Winds and wind shear cannot directly influence CAPE. Did you mean to say that winds and wind shear affect the propensity for tropical cyclogenesis? Low vertical wind shear is one of the conditions identified by Gray (1975) as being necessary for cyclogenisis. [Matthew C. Wheeler]	
3-1743	A	69:1	69:2	See also GHG forced model projections by Knutson, T.R. and R.E. Tuleya, 2004: Impact of CO2-induced warming on simulated hurricane intensity and precipitation: sensitivity to the choice of climate model and convective parameterization. J. Clim., 17, 3477-3495.	

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				[Christian-D. Schoenwiese]	
3-1744	A	69:12	70:33	It could be helpful to bring in more of the material from the newly published papers by Emanuel and by Webster et al. on hurricanes/typhoons. It might be possible to bring Figures 1, 2, 3 of Emanuel into a composite with Figure 3.8.3. In any case, more discussion of those papers could be useful, noting in more detail their strong points and remaining questions. [Susan Solomon]	
3-1745	A	69:12		Section 3.8.3: Tropical and extratropical storms are very different phenomena. I suggest that this section be split in two section, one on Tropical storms and one on Extratropical Storms. [Fons Baede]	
3-1746	A	69:25	69:25	Remove the word "Atlantic". ACE can account for changes in intensity in any ocean basin. [Matthew C. Wheeler]	
3-1747	A	69:40	69:40	empirical rules"… change to "theories [Thomas Knutson]	
3-1748	A	69:42	69:45	Text implies Gettleman et al. is not subject to the same data quality issues as DeMott and Randall, but their basic data sources are the same, so similar caveats should apply to both (as well as to Free et al.). [Melissa Free]	
3-1749	A	69:42	69:43	Change "tropical radiosonde stations around the globe" to "radiosonde stations throughout the tropics" [Adrian Simmons]	
3-1750	A	69:43		The positive CAPE trends found by Gettelman 2002 are probably due to trend biases in sonde temperatures as reported by Sherwood et al. 2005, which increase strongly with altitude. [Steven Sherwood]	
3-1751	A	69:46	69:46	"although it seems clear that trends in PI are small…" Why is the Free et al study singled out as providing a highly reliable assessment given the problems mentioned earlier in the paragraph? [Thomas Knutson]	
3-1752	A	69:49	69:54	It should be pointed out that while Emanuel (2005) purports to find a trend towards unusually elevated power dissipation index values (that he defines as an indication of destructiveness) there has been no documented increase in observed destructiveness of tropical storms (Pielke, Jr. submitted to Nature) and there have been objections raised to his methodology (Landsea, submitted to Nature) and his overall conclusions in both the	

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				Atlantic (Landsea submitted to Nature) and Pacific (Chan submitted to Nature) basins. Obviously for a work to raise such numerous objections by leading scientists suggests that the results and conclusions are far from being agreed upon at the current time. [Jeffrey Kueter]	
3-1753	A	69:49	69:57	It should be noted that both of these studies find trends in major cyclones with surface temperature that far exceed what would be predicted theoretically; and that the trends require significant adjustments to the data for homogeneity which is a potentially large source of uncertainty.Also, this whole section should be checked to make sure that terminology such as "hurricane," "tropical cyclone" and "tropical storm" are used consistently and correctly (the latter two seem to be used interchangeably though they are not equivalent, and the first is often used where I believe the second may be meant). [Steven Sherwood]	
3-1754	A	69:51	:54	I suggest to add a figure to show the "substantial upward trend" of the Power Dissipation Index, in particular for the Atlantic region. [Fons Baede]	
3-1755	A	69:51		How reliable is v^3? [Gabriele Hegerl]	
3-1756	A	69:54	69:56	Text should note that Webster (2005) begins in 1970, and that if he had studied Atlantic back to 1944he would have found that the relative frequency of Category 4 and 5 storms in the 1940s and 50s is the same as it is now. Critics of hurricane/warming linkages are using this argument successfully on TV; that means it better be attended to here. [Jeffrey Kueter]	
3-1757	A	70:18	70:19	Text should note that Webster (2005) begins in 1970, and that if he had studied Atlantic back to 1944he would have found that the relative frequency of Category 4 and 5 storms in the 1940s and 50s is the same as it is now. Critics of hurricane/warming linkages are using this argument successfully on TV; that means it better be attended to here. [Jeffrey Kueter]	
3-1758	A	70:19		The forthcoming paper of Pielke Jr., R. A., C. Landsea, M. Mayfield, J. Laver and R. Pasch, 2005: Hurricanes and global warming. Bulletin of the American Meteorological Society, 86, (in press) should be cited (and commented upon?) here. [Ian Simmonds]	
3-1759	A	70:21		Figure 3.8.3: I find myself trying to eyeball average the different regions - would that be ok to do, and if yes, could you show it? [Gabriele Hegerl]	
3-1760	A	70:21		Says "Insert Fig. 3.8.3, and so does page 3.71, line 41. Thus, Fig. 3.8.3 is said to be	

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				inserted twice, once on page 3-70 and once one page 3-71. [Melinda Marquis]	
3-1761	A	70:24	70:31	The text could be formulated such as :'In the western North Pacific, no evident long-term trend for the annual total number of typhoon /tropical cyclones can be identified, due to a strong inter-decadal variation (Figure 3.8.3). This statement has been documented in a number of studies (……)", insert all references. [Aristita Busuioc]	
3-1762	A	70:45	70:45	Change "The 21 .." to "The number of 21 .." [FILIPPO GIORGI]	
3-1763	A	70:49	71:39	I would like to see Emanuel's diagrams of SST versus total hurricane power dissipation shown for the North Atlantic. I believe that very recently Emanuel has extended this time series back in time, giving a longer perspective. An update to include 2005 should be a high priority. [Chris Folland]	
3-1764	A	70:50	70:56	The text related to 3.8.3.1.2 is too long and not well summarised, some passages being repeated (for example between lines 50-54/pag.70 and 5-7/pag.71). The text should begin with a general statement such as "The hurricane activity in this region is characterised by multi-decadal fluctuations…." [Aristita Busuioc]	
3-1765	A	70:50	71:39	this section should include information on the frequency and trend in tropical cyclones over the entire 20th century. Also a supporting figure should be added for this time period. [Howard Feldman]	
3-1766	A	71:8	71:9	Need to explain here that there are actually three possible categories (above normal, below normal, and normal), otherwise the reader will assume there are just two and the numbers don't add up. [Dian Seidel]	
3-1767	A	71:12	71:13	"Unprecedented four hit Florida" [in 2004]. This frequency of strike for such an area is not unprecedented (unless, for some reason, the rhetorical use of Florida is important, which is very unscientific and very political). Take a look at the Mexican Gulf Coast north and west of the Yucatan; it had 7 strikes in 1933. Either remove the Florida reference or note the strike frequency in Mexico. [Jeffrey Kueter]	
3-1768	A	71:13	71:15	Same as previous comment. The sentence should be rewritten accounting for the exceptional 2005 hurricane season in the north Atlantic [Piero Lionello]	

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3-1769	A	71:13	71:15	Update very important, realizimg the extreme hurricane season in 2005. [Christian-D. Schoenwiese]	
3-1770	A	71:13	71:15	I'm sure the reamrks about the 2005 hurricane season will be updated carefully in the light of events in the Gulf States of the USA. [Adrian Simmons]	
3-1771	A	71:29	71:29	…phase of the AMO. Knutson et al. (2006) find that warming trends in this region over the 20th century exceed that expected from internal climate variability or natural climate forcings, but are relatively consistent with a coupled climate model 20th century simulations that include anthropogenic forcing. Reference to add: Knutson, T.R., T. L. Delworth, K. W. Dixon, I. M. Held, J. Lu, V. Ramaswamy, M. D. Schwarzkopf, G. Stenchikov, and R. J. Stouffer (2006): Assessment of Twentieth-Century Regional Surface Temperature Trends using the GFDL CM2 Coupled Models. J. Climate, accepted for publication. [Thomas Knutson]	
3-1772	A	71:33	71:35	The paper by Chan and Liu (2004) does not mention CAPE. This is either an incorrect reference, or CAPE is being used incorrectly in this context. [Matthew C. Wheeler]	
3-1773	A	71:38	:39	I suggest to add a figure here to show the "substantial upward trend" of the Power Dissipation Index, in particular for the Atlantic region. [Fons Baede]	
3-1774	A	71:45	71:41	I suggest to delete "Quasi-Biennial Oscillation" and introduce this explanation of QBO on page 27, line 34, where QBO is used first time in this chapter. [ILEANA MARES]	
3-1775	A	71:45	71:45	Here the QBO is mentioned with regard to Pacific cyclones, but it was never mentioned in the discussion of the Atlantic. Should it be? [Dian Seidel]	
3-1776	A	71:50	71:50	Specify why the " the seasonal ACE values are unreliable prior to 1970? [Aristita Busuioc]	
3-1777	A	72:23	72:24	I would dispute this - we have pretty consistent data since about 1970, and trends have been determined. [Neville Nicholls]	
3-1778	A	72:26	72:33	Whether or not the March 2004 South Atlantic storm Catarina was true tropical cyclone, whether it was unique in the historical record, and whether or not it is related to climate change are all unsettled issues. For example, the water temperatures over which it formed were actually a bit cooler than normal. And further, the Spring 2005 UCAR Quarterly,	

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				(http://www.ucar.edu/communications/quarterly/spring05/index.html) reports on efforts by Lance Bosart to assemble a research team in order to look into the historical records to see whether there have been any other Catarina-type storms in Brazil. Not enough mention is made of the difficulties and/or ongoing investigations into assessing the true historical nature of Catarina. [Jeffrey Kueter]	
3-1779	A	72:26	72:33	Section 3.8.3.1.6 Lines 27 and 31 are sort of contradictory. There is a lot on uncartinity in saying that the Catarina was the first and only hurricane in the region. There are not statitics of the number of events in the region. Some information indicates that something similar may have happened in 1970, but no records of intensity are available. This does not indicate that any other similar event could have happen in the past. [Jose Marengo]	
3-1780	A	72:35	72:35	In October 2005, for the first time a tropical cyclone appeared southwest of the coast of Portugal, that means in extratropical regions. Although this was a relatively weak and short-lived phenomenon, it is worth to mention (for more information please contact Weather Servive of Portugal). [Christian-D. Schoenwiese]	
3-1781	A	72:40	72:40	Add "Wang et al., 2005a" after "McCabe et al., 2001". [Xiaolan L. WANG]	
3-1782	A	72:47	72:47	Add "Wang et al., 2005a" before "Graham and Diaz, 2001; …". [Xiaolan L. WANG]	
3-1783	A	72:48	72:48	It is not true that "only the North Pacific trend is found statistically significant". Wang et al. (2005a) also found a significant increasing trend over the high latitude North Atlantic, with a significant decreasing trend over the mid-latitude North Atlantic, in addition to an increasing trend in strong-cyclone activity over the mid-latitude North Pacific. [Xiaolan L. WANG]	
3-1784	A	73:0	74:	Section 3.8.3.3. A short paragraph should be added in this section to report on observed trends in the frequency of freezing precipitation and blowing snow occurrence in Canada (Wang, 2005; Hanesiak and Wang 2005), since these events are as relevant as dust storms in this section. Thus, I suggest add the following (or the like) as the fourth paragraph of this section: "A significant increasing trend in the frequency of freezing precipitation was observed in northern Canada, with a decreasing trend in some areas of southern Canada, and a significant decreasing trend in the frequency of blowing snow occurrence everywhere across Canada (Wang, 2005; Hanesiak and Wang, 2005). These trends were found by the same authors to be physically consistent with trends seen in cyclone activity, air temperature, and precipitation over Canada." [Xiaolan L. WANG]	

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3-1785	A	73:0		Tornadoes, hail, thunderstorms, dust storms, and other local severe weathe imply a huge challenge.. This level of short space- and time-scale focus looks to likely prove to be a statistical nightmare. Dump it for now. [Jerry Mahlman]	
3-1786	A	73:11	73:27	Using hourly Canadian station pressure data for 1953-2002, Wang et al. (2005b) found that winter cyclone activity has become more frequent/durable and stronger over the lower Canadian Arctic, but less frequent and weaker in southern Canada, that summer cyclone activity has become more frequent in the Canadian east coast, and that cyclone activity over Canada is closely related to major teleconnection patterns (especially the NAO). Something like this should be added in this paragraph (maybe before the sentence "Alexandersson et al. (2000)...". [Xiaolan L. WANG]	
3-1787	A	73:26	73:26	Sentence reads oddly. Add " in severe storms" after "increase". [Chris Folland]	
3-1788	A	73:29	73:41	Please insert (with possible editing) in this paragraph: After homogenization the near-surface wind data (accounting for different anemometer he+G11ights), Groisman et al. (2004) reported a nationwide reduction (~1% per decade) of near-surface wind speed at the airports over the conterminous United States in the second half of the 20th century. However, they warned that this reduction could be not only due to the climatic factors but also due to changes in the surface roughness around the airfields (e.g., due to urbanization and tree growth) that was not controlled (accounted for) in their analysis. [Pavel Groisman]	
3-1789	A	73:43		A summary paragraph on changes in tropical storms would be very useful [FILIPPO GIORGI]	
3-1790	A	73:45	74:8	I am Contributing Author for WG1 Ch3, Sec. 3.8.3.3. For this review phase, I have prepared an updated version of that Section, which I will submit to my LA and the TSU in a separate eMail. [Dr. Nikolai Dotzek]	
3-1791	A	73:45		Lightning trends might be a better/ more readily available index of changing convective intensities. [Robert E. Dickinson]	
3-1792	A	73:47		Even though the tornado data bases in the US are "well established", it is important to point out some new results since TAR: "There have been no statistically significant in stronger tornadoes (F2 or greater) within the United States, and there is only weak ENSO related variability (Marzban and Schaefer, 2001; Wickle and Anderson, 2003). Any ENSO related variability seems to be more a function of higher activity shifting spatially rather than temporal variability. || Marzban, C, and J.T. Scaefer, 2001: The correlation	

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				between U.S. tornadoes and Pacific Seas Surface Temperatures. Mon. Wea. Rev., 129, 884-895. || Wikle, C.K., and C.J. Anderson, 2003: Climatological analysis of tornado report counts using a heirarchical Baysean spatio-temporal Model. J. Geophys. Res., 108 (D24), 9005. [Anthony Lupo]	
3-1793	A	73:52		cite "(Feuerstein et al., 2005)" [Jürgen Grieser]	
3-1794	A	74:0	81:0	My exhaustion is now a critical factor here, but if a Chapter author wishes to plow through all of these historical "climate events" with a diagnostic eye, considerably more care and attention than that of a cursory overview is an absolute necessity to achieve success. [Jerry Mahlman]	
3-1795	A	74:0		Box 5 specific Extremes events, There are any specifications for North africa,Many extreme events are observed since 1980, several droughts and floods as Algiers in Nov 2001(750 dead) [Matari Amar]	
3-1796	A	74:10		I really liked this section on recent extreme events. [Dian Seidel]	
3-1797	A	74:12	76:36	Very useful box on extreme events. Is it useful doing another paragraph on the 2005 North Atlantic hurricane season? Although the jury is out, in terms of record length, for observation evidence of trends in intensity, a focus on the record breaking season in terms of frequency and intensity would be of considerable interest. [M James Salinger]	
3-1798	A	74:12		Many extremes events are observed in North Africa (Maghreb) since 1980, several drought and floods as Algiers in November 2001 (750 dead) [Amar Matari]	
3-1799	A	74:14	74:22	Box 3.5. This box on specific extreme events is totally inappropriate. Why not also include a box on the extreme events that occurred from 1896-1905? Or 1926-1935? Or any other period? Clearly, you can cherry-pick events from any period in history that conform to some expectation of climate change that you trying to illustrate. Why didn't you describe any specific extreme events that ran opposite to your climate change expectations? [Jeffrey Kueter]	
3-1800	A	74:14		Box 3.5 Some brief indications about the effects of these five specific extreme events would be very appropriated (e.g. deaths by the heat wave in Europe, summer 2003, in France). [Javier Martin-Vide]	

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3-1801	A	74:17	74:18	I do not agree with the statement "examples are given … unchanging climate". As far as I know nobody has really shown that climate change has made these individual specific events more likely (with the exception perhaps of the 2003 European drought heat wave, although even there the connection is weak).There is an implied attribution statement here that I do not agree with. I would replace this sentence with something like "types of events whose frequency might be expected to increase in future climate change conditions". In the latter case no attribution is implied. [FILIPPO GIORGI]	
3-1802	A	74:19		Add "term" after "longer" [Adrian Simmons]	
3-1803	A	74:56	75:1	These statistics are misleading. Record daily maximum temperatures for Adelaide, Melbourne and Sydney were set sequentially in January 1939 which establishes this as the Southeast Australian heat wave of note. The 2002 drought was one year in an extended period of below average rainfall over southeastern Australia between 1997 and 2005. [WILLIAM KININMONTH]	
3-1804	A	75:1	75:1	Ref. comment #2 above: "evaporation" should read "evapotranspiration" [Michael Hobbins]	
3-1806	A	75:21	75:26	This paragraph (though interesting) has, I believe, no place in this chapter. It should be checked that what it says is covered by what is being written somewhere else on impacts. [Adrian Simmons]	
3-1807	A	75:35	75:35	Problem with Hoerling and Kumar (2003) is that there is no correlation between western U.S. PDSI (or U.S. PDSI) and warming, as anthropogenic greenhouse forcing increased since 1950. As it stands, the text conflates the recent western drought (two paragraphs previous) with this study. Text needs to note that in fact there is no significant relationship between PDSI and drought in that region. Insert “Despite no apparent statistical relationship between western North American PDSI values and planetary temperature, Hoerling and Kumar….” [Jeffrey Kueter]	
3-1808	A	75:35	:41	This has overlap with ch9, which is ok, maybe should refer to our discussion, also, and make sure we stay consistent [Gabriele Hegerl]	
3-1809	A	75:43	75:43	Figures Question 3.2, Figure 1 and Figure 3.8.5. The latter shows increasing El Nino precipitation in the American southwest, the former claims abnormal drought at the same time. Please adjust the text to explain this seeming contradiction. [Jeffrey Kueter]	
3-1810	A	75:48		from extraordinary" instead of "from this extraordinary [Jürgen Grieser]	

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3-1811	A	75:52	75:52	"Ore Mountains" would be preferable. [Martin Stendel]	
3-1812	A	75:57	76:1	"Ore Mountains" would be preferable. [Martin Stendel]	
3-1813	A	76:5	76:5	"Ore Mountains" would be preferable. [Martin Stendel]	
3-1814	A	76:15	76:32	Cross refer to the Detection and Attribution Chapter as this is a well known, possibly first example, of regional anthropogenic climate change detection within one event. The paper by Stott et al refers: Stott, P. A., Stone, D. A., and Allen, M. R., 2004: Human contribution to the European heatwave of 2003. Nature, 432, 610-614. [Chris Folland]	
3-1815	A	76:15	76:32	Using the new extreme analysis technique mentioned above (comment 28), German summer temperature data 1761-2003 were analysed. The result is that such an extreme event like the summer heat wave 2003 was nearly impossible to occur until roghly 1970/80, but since that time the occurrence probability increased by a factor of approximately 20. Nevertheless, when occurring, it was, statistically seen, a very rare 455 yr event. Reference: Schönwiese, C.-D., R. Staeger and S. Trömel, 2004: The hot summer 2003 in Germany. Some preliminary results of a statistical time series analysis. Meteorol. Z., 13, 323-327. [Christian-D. Schoenwiese]	
3-1816	A	76:17	76:19	In our recent study Brunet et al. 2005 (already quoted in this Chap.) the estimated mean anomalies (re 1961-1990 period) for June and August over mainland Spain also showed these months like the warmest months across the 1850-2003 period. So, please, consider incorporating after Schär et al., 2004; "and Brunet et al. 2005 over mainland Spain"). [Manola Brunet]	
3-1817	A	76:17	76:21	Please, add the following result: Also in southern Europe (Italy) the 2003 summer was the warmest of the last two centuries, with values 4.8 and 4.0 K above the 1961-1990 normal, for maximum and minimum temperature respectively (Brunetti et al., 2005). REFERENCE: M. Brunetti, M. Maugeri, F. Monti, T. Nanni. 2005. Temperature and precipitation variability in Italy in the last two centuries from homogenized instrumental time series. International Journal of Climatology, in press. [Michele BRUNETTI]	
3-1818	A	76:17	76:34	The rise of summer (JJA) temperature in Central Europe is remarkable over the past 30 years. This feature should be emphasized in the discussion. [Henry Diaz]	
3-1819	A	76:17	76:21	I would suggest to highlight that also in Italy the summer of 2003 was the warmest of the last two centuries, with average temperature 4.4 K above the 1961-1990 normal value	

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				(Brunetti et al., 2005). REFERENCE: Brunetti, M., Maugeri, M., Monti, F., Nanni T., 2005: Temperature and precipitation variability in Italy in the last two centuries from homogenised instrumental time series. Int. J. Climatol., in press. [Teresa NANNI]	
3-1820	A	76:19		After "…3.8.6." add: "An analysis of temperatures over a 3300 m altitudinal range in the Alps shows that hot nighttime temperatures were particularly frequent at low elevation whereas hot daytime highs were correlated with the average degree of insolation of the sites (Rebetez, 2004)." [Rebetez, M, 2004: Summer 2003 maximum and minimum daily temperatures over a 3300 m altitudinal range in the Alps. Clim. Res. 27: 45-50] [Martine Rebetez]	
3-1821	A	76:21		After "WMO, 2003)." add: "Insolation was above normal, particularly in Northern or mountainous regions which usually have rainy and cloudy episodes in summer (Rebetez et al., 2005)." [Rebetez M, Mayer H, Dupont O, Schindler D, Gartner K, Kroppe J, Menzel A, 2005: Heat and drought 2003 in Europe : a climate synthesis. Ann. For. Sc., accepted] [Martine Rebetez]	
3-1822	A	76:24	76:25	This statement holds only for Switzerland (Luterbacher et al., 2004). [Christian-D. Schoenwiese]	
3-1823	A	76:25	:27	"..that some weather services suggest…" - are there any references available to support this statement? [John Caesar]	
3-1824	A	76:26		After "…high pressure system", quote "(Black et al., 2004; Fink et al., 2004)" [Black, E., M. Blackburn, G. Harrison, B. Hoskins, and J. Methven, 2004: Factors contributing to the summer 2003 European heatwave. Weather, 59, 217-223.] [Fink, A. H., T. Brucher, A. Kruger, G. C. Leckebusch, J. G. Pinto and U. Ulbrich, 2004: The 2003 European summer heatwaves and drought – synoptic diagnosis and impacts. Weather, 59, 209-216.] [Martine Rebetez]	
3-1825	A	76:32		This has previously been discussed by Black et al (2004) and Schonwiese et al. (2004). Black, E., M. Blackburn, G. Harrison, B. J. Hoskins and J. Methven, 2004: Factors contributing to the summer 2003 European heatwave. Weather, 59 (8), 217-223 Schonwiese CD, Staeger T, Tromel S, 2004: The hot summer 2003 in Germany. Some preliminary results of a statistical time series analysis. METEOROLOGISCHE ZEITSCHRIFT 13 (4): 323-327 [Christoph Schar]	
3-1826	A	76:34		Figure 3.8.6 should be changed, or at least its caption. It shows nice smooth Gaussian	

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				curves representing the distribution of maximum temperatures. These presumably (it is not stated) have been obtained by fitting the data, but one wonders how Gaussian is the actual distribution. I'd prefer to see the actual distribution shown, even if it takes some ingenuity with the graphics This figure looks at odds with the many others in the chapter that show the underlying values as well as smoother fitted curves. [Adrian Simmons]	
3-1827	A	76:38	76:53	Please draw together here a few quantitative conclusions on trend in extremes, especially from the Alexander et al quasi-global results. [Chris Folland]	
3-1828	A	76:38	76:38	Paragraph 3.8.4 is summary for 3.8, but not all sections have a summary put in separated paragraphs. [ILEANA MARES]	
3-1829	A	76:41	76:43	Delete this sentence referring to gradual reduction in numer of frost days. See comment 67. [Adrian Simmons]	
3-1830	A	76:42	76:44	There has been no prior discussion on heat wave duration changes, and the prior discussion of warm extremes has indicated that they have changed little, so where does the line “In agreement with this warming trend, the number of warm extremes has also increased, and heat waves have increased in duration” come from? [Jeffrey Kueter]	
3-1831	A	76:55	77:8	Its not true that trends are small compared to the variability for the new Emanuel (2005) results on hurricane power, at least in some areas like the North Atlantic. This is only one paper of course but should be mentioned here. [Chris Folland]	
3-1832	A	76:55	77:8	"any trends are small compared with the variability". This seems at odds with the discussion earlier in the chapter and with the findings of Emanuel et al. (2005) and Webster et al. (2005). [Nathan Gillett]	
3-1833	A	77:1	77:1	"is evident that any trends are small compared with the variability…" This statement seems at odds with Emanuel (2005), especially considering resutlts he presents for the NW Pacific basin or combined Atlantic/NW Pacific basin. [Thomas Knutson]	
3-1834	A	77:5	77:5	1997/98, not 1997. [Christian-D. Schoenwiese]	
3-1835	A	77:10	77:16	As stated in reponse to Section 3.3.3, using PDSI to examine trends in droughts is fundamentally flawed.	

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				[Michael Hobbins]	
3-1837	A	77:10	77:16	This paragraph is repeated almost line for line in the question. And expanded upon! I don't think the paragraph is therefore required. [Peter Thorne]	
3-1838	A	77:11	77:12	According to Table 3.4 the 'decreases in land precipitation since the 1950s' are only weekly (statistically) significant. [Javier Martin-Vide]	
3-1839	A	77:20	77:29	This statement needs to be clearly referenced as to what period one is talking about. Espcially the statement, "droughts also have increased …" Droughts come and go. There have been worse droughts in many parts the world recorded in the last 150 years. In the US, for instance, the Dust Bowl droughts of the 1930s still stand as the worse on record. [Henry Diaz]	
3-1840	A	77:20	77:21	after "heat waves" I sugggest to add "cold waves" [Philippe Tulkens]	
3-1841	A	77:20	78:27	This section particularly needs cross refering to the Detection Chapter (and vice versa). particularly how to deal with the 2003 European heat wave. Again I would like to see more quantitative discussion of extremes trends from previous parts of the chapter [Chris Folland]	
3-1842	A	77:20		Question 3.3: I suggest that in the answer to this and all other Questions the use of acronyms and too technical terms be avoided. [Fons Baede]	
3-1843	A	77:23	77:25	But elsewhere in this chapter, precipitation has been reported as widely increasing. [Michael Hobbins]	
3-1844	A	77:23	77:25	Again, ETa increasing due to warmer conditions. As pointed out earlier, the causality of warming leading to drying is so weak that observations of the same are mere correlation. [Michael Hobbins]	
3-1847	A	77:23		I would suggest that the answer states explicitly whether the intensity and frequency of cold waves has decreased or not over the last decades. Some elements of answer are given but cold waves could be addressed specifically. [Philippe Tulkens]	
3-1848	A	77:24	77:24	Ref. comment #2 above: "evaporation" should read "actual evapotranspiration" [Michael Hobbins]	
3-1850	A	77:24		The time period to which these claims pertain should be explicitly noted (e.g., "Over the last five decades or so, ...") Most trends other than temperature have not been consistent over the last 100 years.	

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				[Steven Sherwood]	
3-1851	A	77:36	77:38	Notes on tendancies in extremes are at odds with the section describing changes in extremes. [Peter Thorne]	
3-1852	A	77:37	77:37	Should read "…shape, such that the cold extremes….". [John Caesar]	
3-1853	A	77:42	77:43	Here the text refers to “moderate to heavy precipitation events” increasing. This is correct. TAR uses 50mm/day and the upper 5 percentiles as “heavy and extreme precipitation”. "Heavy" may apply, but “extreme” surely does not apply to one-in-twenty rain events, and it should be dropped. [Jeffrey Kueter]	
3-1854	A	77:42		Question 3.3: Suggest citing Q3.2 here which has very similar statements. [David & David Wratt & Fahey]	
3-1855	A	77:47	77:47	"Drought is by far the simplest extreme to measure." I absolutely disagree. For a start, floods are a far more easily observed extreme event than are droughts: they have a simple, objective definition and a clearly observable start and conclusion. Droughts, on the other hand, have as many definitions as there are stakeholders--a drought to a irrigator may be nothing of the sort to a municipal water supplier, or to a wildlife biologist, or to a forester, or to a wildland firefighter, or to a ski-are operator, etc., not only because the quantities of water with which these disparate users are concerned are so different but also because the form of water and therefore the metric of their dought can range from transpiration, streamflow, groundwater flow, precipitation, soil moisture, etc. Further, it is often very hard to watch a drought develop from inception as they only become detectable when already well underway. Further still is the issue of defining the spatial boundaries of a drought. Anyway, my point is that droughts are far indeed from being the simplest extreme to measure. Just because we have a simple metric to measure atmospheric drought (the PDSI) doesn't make analysis of drought simple--far from it, PDSI makes the poor analysis of drought simple. [Michael Hobbins]	
3-1856	A	77:47	77:57	Ref. comment #5 above regarding the worth of PDSI as a drought measure. [Michael Hobbins]	
3-1859	A	77:47	78:2	The study by Dai et al. (2004) used the Thornthwaite approach to estimating potential evaporation. This is wrong. This section needs to be deleted awaiting the results of new research that uses a more appropriate estimate of potential evaporation. See comment 8. [Michael Roderick]	
3-1860	A	77:55	77:56	Here we see the faulty brownhouse scenario--that increasing Tair suggests drying--again. [Michael Hobbins]	

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3-1862	A	77:55		Given the dubious statistical significance of the downward precip trends over the last 50 years, it does not seem justifiable to say that these "are the main cause for the drying trends." [Steven Sherwood]	
3-1863	A	77:56	77:56	also likely contributed to the drying" should more properly read "also likely contributed to the apparent drying [Michael Hobbins]	
3-1865	A	78:5	78:15	Suggest motivating/explaining the use of El Nino and NAO concepts. [David & David Wratt & Fahey]	
3-1866	A	78:5		Change "stroms" to "storms". [Adrian Simmons]	
3-1867	A	78:9	78:10	The statement on potential destructiveness trends should be modified to include the fact the rise from the mid-1970s to the present is largely (if not entirely) a result of the evolution from the low extreme of the AMO to the high extreme of the AMO. [Jeffrey Kueter]	
3-1868	A	78:16	78:18	Re-write as: "Observational evidence for changes in small-scale severe weather phenomena (such as tornadoes, hail and thunderstorms) is mostly local and too scattered to draw general conclusions; increased number of reported events (especially weaker ones) in many areas simply arises because of greater public awareness and better information flow due to the internet since about 2000, or are due to population density variations or increased vulnerability. [Dr. Nikolai Dotzek]	
3-1869	A	78:20	78:27	This paragraph makes a good and essential point. It should be repeated prominently at the beginning of the section on climate extremes, rather than appearing for the first and only time at the end of Question 3.3 [Jeffrey Kueter]	
3-1870	A	78:20	78:27	I am not sure what the point of this paragraph is. Perhaps it could be revised so that is says, basically, "Changes in how people live have made us much more vulnerable to, and aware of, weather extremes. This can produce an exaggerated impression that the wrath of nature is rapidly worsening. Nonetheless, careful and objective analysis of many types of data does show that in many important respects weather has indeed become somewhat more severe in recent decades." Or something like that. [Steven Sherwood]	
3-1871	A	78:20	78:27	Aside from the above comment, does this paragraph really have a place here. Should not	

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				the worthy points it makes be made where impacts are discussed? If it remains, the general writing needs to tightened up, to distinguish (cf last line) between "weather disasters" and "climatic disasters". What was Katrina? A "weather disaster", a "climate disaster" or an "administrative disaster"? [Adrian Simmons]	
3-1872	A	78:20	78:27	The last paragraph of answer to Question 3.3 leaves the reader with the impression that, after all, the increase of extreme events is more of an impression than a reality. This tends to undermine the outcome of the extensive data analysis described in Chapter III. The media and socio-economic elements cited are appropriately acknowledged. However, in this section of the IPCC report, I would expect a clear message on whether it is more of an impression or if the data support the hypothesis of increasing frequency/amplitude extreme events [Philippe Tulkens]	
3-1873	A	78:20		Insert "weather" before "extremes". Some climate extremes (eg droughts) do not have a single point of occurrence (though equally eventually attract the eye of the media). See comment 64 as to what might or might not be regarded as a climate extreme. Without qualification, the sentences in question on pages 64 and 78 are a bit inconsistent. [Adrian Simmons]	
3-1874	A	78:33		Section 3.9. I have strong reservations about the appropriateness of this section. In most cases it is not possible to say that things are consistent without using a model of some kind. In some cases this is an AOGCM, so the conclusion is not really an observational one at all. Even when you are not using an AOGCM, you have some physical model in mind which could relate the observations. To assert that these conclusions are observational is to bypass the properly physically or statistically based rigour of AOGCMs and D&A studies. It's not properly justified. Where the link is so basic that you can maintain that it is observational, it could equally well be put somewhere earlier in the chapter, remarking "This fact Y is consistent with the earlier fact X." Otherwise I think the discussion of consistency should be in chapter 9, following the discussion of AOGCMs in chapter 8. [Jonathan Gregory]	
3-1875	A	78:35		Synthesis. A brief comment may be needed on the recent enhanced vegetation activity and lengthened growing season (as observed by NDVI) over northern middle-high latitudes in relation to the temperature and precipitation trends. [Masato Shinoda]	
3-1876	A	78:35		Section 3.9. The consistency of observation is done with chapter 4 and 5. It should be done also with some elements of chapter 2 such as water vapour increase and its radiative effect. (see section 2.3.8)	

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				[Philippe Tulkens]	
3-1877	A	78:36		Add "the realism of apparent" before "recent observed changes". [Adrian Simmons]	
3-1878	A	78:38	78:39	The moisture holding capacity of the atmosphere is the difference between the saturated vapor pressure (a function of Tair) and the actual vapor pressure (a function of Tdew). Thus, when Tair increases alone, it will, as stated here (and throughout this chapter), indeed increase the moisture-holding capacity of the air. However, Tair never increases alone. In fact, over the last 50 years across the conterminous US, Tdew has increased at over twice the rate Tair has increased, thereby reducing the moisture-holding capacity of the air (Hobbins et al., 2004). [Michael Hobbins]	
3-1880	A	78:45	78:46	Can you estimate an uncertainty for this type of non linear trend? The linear trend and its uncertainty should also be mentioned, with an assessment of which trend number is overall better (if either), a judgement likely needed for the Policymakers' Summary. [Chris Folland]	
3-1881	A	78:45	78:46	I strongly recoomend to relate only to linear (or non-linear) trends, not to filtered data (see above, comment 11) [Christian-D. Schoenwiese]	
3-1882	A	78:45	78:50	Please check and clarify that statements that 'some areas have not warmed, and a few have cooled but not significantly.' This appears to be inconsistent with statements made elsewhere about cooling over the Antarctic plateau (at least in some seasons?). It would also be helpful to clarify if the cooling near Greenland shown in TAR is not significant in the most recent data, and how the behavior of the eastern US is to be interpreted. [Susan Solomon]	
3-1883	A	78:45	78:45	Why was the figure of 0.75 K chosen here rather than the other value of 0.6 K given in the executive summary ? [Philippe Tulkens]	
3-1884	A	78:45		0.75K is incorrect as it applies to 1860-2004 (see page 3_3 line 8) Replace “0.75K” with “0.57 to 0.626K”. See Table 3.3 [Vincent Gray]	
3-1885	A	78:46	78:46	athough rates….." may be changed in "and greatest rates of temperature rise are observed after 1979 [Franco Desiato]	
3-1886	A	78:46	78:49	Slower warming of the oceans is consistent not only “with the much greater mass and thermal inertia of the oceans”, but also with the endothermic effects of evaporation. [David Wasdell]	

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3-1887	A	78:48		Technically the land has more mass than oceans since continental crust is denser and many times thicker than the deepest ocean. Thermal inertia is enough here. [Steven Sherwood]	
3-1888	A	78:50		Add a comma after "cooled". [Adrian Simmons]	
3-1889	A	79:1	79:4	I thought there was an internal conflict to the synthesis bullet point on lines 1-4, page 3-79: "Widespread decreases in continental DTR" appears to disagree with "rate of increase of DTR overall" [Harry Bryden]	
3-1890	A	79:3	79:3	correct "increase" to "decrease" [Reinhard Böhm]	
3-1891	A	79:5	79:6	Extremely misleading. Antarctica's ice mass is growing (Davis et al., 2005, Science) Krabill et al. (2000) give a net change in Greenland of "1 +/- <5 mm/year" inches per year, which is simply not distinguishable from zero. In the very same issue of Science, Thomas et al. wrote "The region as a whole has been in balance, but with a thickening of 21 centimeters per year in the southwest and thinning of 30 centimeters per year in the southeast". And, most recently, Johannessen et al (Science, 2005) reported a substantial increase averaged over most of Greenland, 5.4 cm/year(!). Please change the text to reflect these. [Jeffrey Kueter]	
3-1892	A	79:5	79:18	I tend to favor Jim Hansen's hypothesis that slight decreases in glacier albedo due to soot or other aerosols is melting the glaciers. This possibility must at least be mentioned here. [Steven Sherwood]	
3-1893	A	79:8	79:9	I'm not sure what the "before 1900" means here. As written, the sentence is a statement of the obvious. Does it mean to say that anomalies in the precipitation that fell before 1900 are still a factor in today's fluctuations in glacial extent? [Adrian Simmons]	
3-1894	A	79:13	79:16	Glacial melt phenomena are qualified with the comment " Local temperature records all show a slight warming, but not of the magnitude required to explain the rapid reduction in mass of such glaciers". The anomaly resolves if the endothermic nature of the phase-change from ice to water is taken into account. Radiative heating in the glacier context is taken up only partially by the temperature rise. The energy is invested in the phase-change and is represented by lower ice volume and higher water volume which carries	

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				away the latent heat without significant rise in local temperature. [David Wasdell]	
3-1895	A	79:19		Change "the spring season" to "spring". [Adrian Simmons]	
3-1896	A	79:21		Add "by" before "up to 3K". [Adrian Simmons]	
3-1897	A	79:25		Change "the spring and summer seasons" to "spring and summer". [Adrian Simmons]	
3-1898	A	79:27	79:29	Zwally (Zwally,H.J., Comiso,J.C., Parkinson,C.L., Cavalieri,D.J. & Gloersen,P. (2002) Variability of Antarctic sea ice 1979-1998. Journal of Geophysical Research, 107, 10.1029/2000JC000733.) found a stat significant decrease in Antarctic sea ice over this period for the Antarctic as a whole. [John Turner]	
3-1899	A	79:28	79:30	Misleading. Antarctica shows net cooling (Doran et al., 2002, Nature) since 1966. While most of the continent is south of 65 , most readers do not know this. You can get “no trend over the period” by putting in just enough of the Southern Ocean to balance this out, making a technically correct but rhetorically misleading statement about Antarctica. [Jeffrey Kueter]	
3-1900	A	79:31		Insert “almost” after “are” [Vincent Gray]	
3-1901	A	79:34	79:34	Remove "and also with ERA-40 estimates of lower-tropospheric temperatures". [Qiang Fu]	
3-1902	A	79:38	79:42	These statements are rather too strong given the current state of knowledge of temperature trends in the troposphere, though the evidence is moving in this direction. [Chris Folland]	
3-1903	A	79:38	79:39	Qualitative agreement is a strong statement when there is a factor of two difference. This bullet would stand fine if the words "are in qualitative agreement recording a" were simply changed to "all show". [Steven Sherwood]	
3-1904	A	79:43	79:45	These radiative changes are SMALL and barely detectable, and the link to surface heat storage is not that solid (though certainly encouraging). [Steven Sherwood]	
3-1905	A	79:46	79:51	But some caution should be expressed with respect to the accuracy of humidity observations, not yet fully assessed, as they have known problems in principle.	

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				[Chris Folland]	
3-1906	A	79:50	79:51	"providing a major feedback to radiative forcing [3.4.2]". Is this consistent fith the findings in section 2.3.8. ? I do not recall that the radiative feedback described in section 2.3.8 was "major". It should be checked. [Philippe Tulkens]	
3-1907	A	79:52	80:6	The last sentence of this paragraph is difficult to decipher: are the writers saying that ETa is inferred to be increasing as reflected by the decreasing Epan? This would be an acknowledgement of the complementarity between ETp (as measured by Epan) and ETa, which has been missing everywhere else in this chapter. [Michael Hobbins]	
3-1909	A	79:55	79:55	Ref. comment #2 above: "evaporation" should read "evapotranspiration" [Michael Hobbins]	
3-1911	A	80:1	80:1	Epan is decreasing not only due to the stated decreasing surface radiation but also to increasing Tdew and therefore decreasing VPD (Hobbins et al., 2004). [Michael Hobbins]	
3-1913	A	80:4	80:4	What is "inferred enhanced evaporation"? [Michael Hobbins]	
3-1915	A	80:18	80:24	This assertion that droughts have increased is not warranted by the obserations. The first PC of Fig.1, Question 3.2 reflects the Sahel drought (mostly). The second PC seems to balance dryness/wetness between the SH and NH. I am not persuaded by this statement. [Henry Diaz]	
3-1916	A	80:18	80:24	See comment 17. and 21. [Michael Roderick]	
3-1917	A	80:20	80:20	What is "inferred enhanced evaporation"? [Michael Hobbins]	
3-1918	A	80:20	80:22	Here is the faulty brownhouse scenario--that increasing Tair suggests drying--again. [Michael Hobbins]	
3-1921	A	80:24	80:24	State a phrase about northern Asia where G3dryer conditions were reported during the past 50-100 years [cf., Dai et al. 2004b; Mescherskaya and Blazhevich 1997; Groisman et al. 2005b) [Pavel Groisman]	
3-1922	A	80:25	80:31	Salinity and freshening of the Atlantic were not mentioned in section 3.3 so the bullet point in lines 25-31 on page 3-80 must primarily refer to Chapter 5. I suggest deleting this bullet point here unless section 3.3 is to be expanded to include salinity and Atlantic freshening [Harry Bryden]	

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3-1923	A	80:32	80:38	You don't show that these changes occur in all seasons in both hemispheres, so some qualification is needed here. Its not clear in the Northern Hemisphere that this westerly change continues - earlier results in this Chapter show evidence of a modest recent reversal. [Chris Folland]	
3-1924	A	80:36	80:38	The attribution of the temperature trends in Antarctica to ozone depletion is far greater here (in the summary and conclusions) than it is made in the body of the text (see for example page 57, lines 27-35). The summary is thus overstated. [Jeffrey Kueter]	
3-1925	A	80:39		There's that "climate shift" on line 39 on page 3-80 and here it has the beginnings of a definition, but it is too late. [Harry Bryden]	
3-1926	A	80:41	80:42	There is no statistically significant increase in mean precipitation averaged across the Eastern United States that began in 1977. It is noteworthy that the previous decade was one of the drier ones on the record, including the multi year drought of the 1960s, but there is certainly nothing in the eastern precipitation record that makes the period 1977-2004 stand out from the history back to 1895. Demonstrate a statistically-significant increase averaged, say, in the eastern third of the country, or remove this statement. Figure 3.2.2. supports the conclusion that there is no statistically significant increase. [Jeffrey Kueter]	
3-1927	A	80:52	80:53	Recovery from Mt Pinatubo explains half of the extra rise since 92 according to Church et al Nature, 3 Nov 2005. [Peter Stott]	
3-1928	A	80:52	81:4	Delete this paragraph. Discussion of sea level rise belongs in Chapter 5. [Lenny Bernstein]	
3-1929	A	80:52	81:4	The final bullet point on sea level, lines 52-55, page 3-80 and 1-4, page 3--81 is not discussed at all in Chapter 3. Maybe the issues should be, but at the moment this final bullet point makes a poor ending to Chapter 3 [Harry Bryden]	
3-1930	A	80:52	81:4	I don't think there should be a summary of sea-level here. Chapter 5 is doing this. What is said here is not fully consistent with chapter 5. [Jonathan Gregory]	
3-1931	A	81:1		Change "taken water out of" to "reduced inflow of water to" or "reduced input of water to". [Adrian Simmons]	
3-1932	A	81:6	81:33	The summary is perhaps the most quotable part of the chapter. Perhaps the acronyms	

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				could be dropped from it. [Adrian Simmons]	
3-1933	A	81:11	81:11	After Europe add "and (much of) mid to high latitude Asia". This can be checked in Hurrell's paper published about 1997 on winter temperature advection associated with the increased NAO/NAM. [Chris Folland]	
3-1934	A	81:12	81:14	The attribution of the temperature trends in Antarctica to ozone depletion is far greater here (in the summary and conclusions) than it is made in the body of the text (see for example page 57, lines 27-35). The summary is thus overstated. [Jeffrey Kueter]	
3-1935	A	81:12	81:13	Figure 3.5.1 and page 81, lines 12-13. The text tries to blame the cooling of Antarctica on ozone depletion. But heights are changing significantly, and opposite in sign, (!) in both winter and summer. How does one get such a winter gain when that is when the depletion begins? Please adjust the text to simply note that as a whole Antarctica is cooling and we really don't have a good explanation because of what we see in Figure 3.5.1. [Jeffrey Kueter]	
3-1936	A	81:18	81:23	This could be read as implying that ENSO, the PDO, the AMO, monsoons, and the Hadley and Walker circulation have all responded in well-understood ways to anthropogenic forcing. I think this is too optimistic. For example the expected sign of the ENSO response to anthropogenic forcing is uncertain. [Nathan Gillett]	
3-1937	A	81:18		Change "winds" to "wind systems" [Adrian Simmons]	
3-1938	A	81:22	81:22	Should say the PDO/IPO. [Chris Folland]	
3-1939	A	81:23	81:25	Again in the penultimate paragraph, issues are raised like "THC", "oceans warming at depth" (line 23, page 3-81) and "precipitation increases over the ocean ((line 25, page 3-81) that are not discussed in Chapter 3. Raising them at the end made me doubt that I had read Chapter 3 carefully enough but when I go back they really are not discussed in the main body of Chapter 3. [Harry Bryden]	
3-1940	A	81:26	81:28	Ref. comment #5 above regarding the worth of PDSI as a drought measure. [Michael Hobbins]	
3-1942	A	81:26	81:28	See comment 17. and 21. [Michael Roderick]	
3-1943	A	81:28	81:28	Ref. comment #2 above: "evaporation" should read "evapotranspiration"	

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				[Michael Hobbins]	
3-1945	A	81:30	81:33	I like the final sentence of Chapter 3. [Harry Bryden]	
3-1946	A	81:30	81:33	I do not believe it is possible to assert this from observations alone with such confidence. This kind of conclusion comes from AOGCMs and D&A studies. It is not appropriate to do it in chapter 3. [Jonathan Gregory]	
3-1947	A	82:0	112:	The way the references are reported is not homogeneous and the informations are sometimes incomplete. [Franco Desiato]	
3-1948	A	82:0	112:	References for above text. John R. Christy. 2002: When Was The Hottest Summer? A State Climatologist Struggles for an Answer. Bulletin of the American Meteorological Society: Vol. 83, No. 5, pp. 723–734. John R. Christy, William B. Norris, Kelly Redmond and Kevin P. Gallo. 2006: Methodology and Results of Calculating Central California Surface Temperature Trends: Evidence of Human-Induced Climate Change? Journal of Climate (in press). Christopher A. Davey and Roger A. Pielke Sr.. 2005: Microclimate Exposures of Surface-Based Weather Stations: Implications For The Assessment of Long-Term Temperature Trends. Bulletin of the American Meteorological Society: Vol. 86, No. 4, pp. 497–504. Kevin P. Gallo. 2005: Evaluation of Temperature Differences for Paired Stations of the U.S. Climate Reference Network. Journal of Climate: Vol. 18, No. 10, pp. 1629–1636. Roger A. Pielke Sr. and Toshihisa Matsui: Should light wind and windy nights have the same temperature trends at individual levels even if the boundary layer averaged heat content change is the same? Geophysical Research Letters (in press). [Kevin Gallo]	
3-1949	A	82:0		Suggested additional paper (file Auer-et-al-2001): Auer, I., R. Böhm and W. Schöner, 2001: Austrian long-term climate - Multiple instrumental climate time series from Central Europe. Österr. Beitr. Meteor. Geophys, 25, 147 pp. plus data and metadata CD ("Österreichische Beiträge zu Meteorologie und Geophysik" is the reviewed monograph series of the Central Institute for Meteorology and Geodynamics, Vienna, Austria. If necessary, the English translation would be: "Austrian Contributions to Meteorology and Geodynamics") [Reinhard Böhm]	
3-1950	A	82:0		Suggested additional paper (file Brunet-et-al-2005): Brunet, M, O. Saladié, P. Jones, J. Sigró, E. Aguilar, A. Moberg, D. Lister, A. Walther and D. López, 2005: The	

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				development of a new dataset of Spanish daily adjusted temperature series (SDATS) (1850-2003). Int. J. Climatol. 25: submitted [Reinhard Böhm]	
3-1951	A	82:0		Suggested additional paper (file Brunetti-et-al-2005): Brunetti, M., M. Maugeri, T. Nanni, I. Auer, R. Böhm, W. Schöner, 2005: Precipitation variability and changes in the Greater Alpine Region over the 1800-2003 period. J. Geophys. Res. submitted [Reinhard Böhm]	
3-1952	A	82:0		Suggested additional paper (file Auer-et-al-2005b): Auer, I., et al., 2005b: HISTALP - Historical instrumental climatological surface time series from the Greater Alpine Region. Int. J. Climatol., submitted [Reinhard Böhm]	
3-1953	A	82:0		Suggested additional paper (Boehm-et-al-2001): Böhm, R., I. Auer, M. Brunetti, M. Maugeri, T. Nanni and W.Schöner, 2001: Regional temperature variability in the European Alps 1760-1998 from homogenized instrumental time series. Int. J. Climatol. 21: 1779-1801 [Reinhard Böhm]	
3-1954	A	82:0		references of the same author(s) and the same year should be extended by a, b, … respectively. This happens several times in the manuscript, not only in this chapter [Reinhard Böhm]	
3-1955	A	82:0		ADDITIONAL REFERENCES for CHAPTER 3: (To be inserted appropriately); - Beran, J., Ghosh, S, Sibbertsen, P., 2003: Nonparametric M-estimation with long-memory errors. Journal of Statistical Planning and Inference, 117, 199-205. - Draghicescu, D., 2002: Nonparametric quantile estimation for dependent data. PhD thesis no. 2592, EPFL-WSL. - Ghosh, S., Beran, J., Innes, J., 1997: Nonparametric conditional quantile estimation in the presence of long memory. Student 2: 109-117. - Ghosh, S., Draghicescu, D. , 2002a: Predicting the distribution function for long-memory processes. International Journal of Forecasting, 18, 283-290. - Ghosh, S., Draghicescu, D., 2002b: An algorithm for optimal bandwidth selection for smooth nonparametric quantiles and distribution functions. In, Statistics in Industry and Technology: Statistical Data Analysis based on the L1-norm and related methods, Birkhäuser Verlag, Basel, Switzerland, pp. 161-168. [Sucharita Ghosh]	
3-1956	A	82:16	82:16	Status of the paper is accepted and in press [Enric Aguilar]	
3-1957	A	82:39		Allan R.P. et al" --> "Allan R.P., M.A. Ringer, J.A. Pamment and A. Slingo	

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				[Richard Allan]	
3-1958	A	83:11	83:11	change "Auer, I., et al., 2005" to "Auer, I., et al., 2005a [Reinhard Böhm]	
3-1959	A	83:50		insert: Beck, C., J. Grieser, and B. Rudolf, 2005: A New Monthly Precipitation Climatology for the Global Land Areas for the Period 1951 to 2000. Climate Status Report 2004, pp. 181 - 190, German Weather Service, Offenbach, Germany. [Jürgen Grieser]	
3-1960	A	85:48		Papers cited for addition to the chapter 3 references: [Zhanqing Li]	
3-1961	A	85:48		Chang, F.-L., and Z, Li, 2005a: A new method for detection of cirrus overlapping water clouds and determination of their optical properties, J. Atmos. Sci., 62, 3993–4009, 2005a. [Zhanqing Li]	
3-1962	A	85:48		Chang, F.-L., and Z. Li, 2005b: A near-global climatology of single-layer and overlapped clouds and their optical properties retrieved from Terra/MODIS data using a new algorithm, J. Climate, 18, 4752-4771. [Zhanqing Li]	
3-1963	A	86:57	86:57	Replace "submitted" with "accepted with minor revision" [Aiguo Dai]	
3-1964	A	87:11	87:11	Delete "Comment:" [Aiguo Dai]	
3-1965	A	87:15	87:15	Replaced "Accepted" with "Revised". [Aiguo Dai]	
3-1966	A	87:16	87:17	thick attribute of police [Jean-Marc Moisselin]	
3-1967	A	87:16	87:17	Only the volume number should appear in bold characters. [Philippe Tulkens]	
3-1968	A	88:36	88:36	Add: "El-Shahawy, M.A.: An aerodynamic method to compute evaporation. Indian Journal of Meteorology and Hydrology (Mausam), Vol. 38, 2, 1987." at the start of line 36. [Mohamed El-Shahawy]	
3-1969	A	88:51		insert "Feuerstein, B., N. Dotzek, and J. Grieser, 2005: Assessing a Tornado Climatology from Global Tornado Intensity Distributions. J. Climate, 18, 585-596" [Jürgen Grieser]	
3-1970	A	89:30	89:38	I suggest to put references in an uniform order: single author, 2, 3 authors and et al. (I observed that is the rule for this chapter)	

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				[ILEANA MARES]	
3-1971	A	89:32	89:32	Blister" should be "Bister [Melissa Free]	
3-1972	A	89:37	89:37	"submitted" should be "in press" (publication online should occur by the end of Nov. 2005) [Melissa Free]	
3-1973	A	89:44	89:50	Same suggestion as above [ILEANA MARES]	
3-1974	A	91:25		insert: Grieser, J., S. Trömel, and C.-D. Schönwiese, 2002: Statistical time series decomposition into significant components and application to European temperature. Theo.Appl.Clim., 71, 171-183. [Jürgen Grieser]	
3-1975	A	91:41	91:41	The current status of the manuscript is: "tentatively accepted pending the acceptance of the entire volume by the Chief Editor of the Journal". {This is a special thematic issue handling by the guest editors]. [Pavel Groisman]	
3-1976	A	91:42	91:43	Insert here as Groisman et al. (2005c) reference Groisman et al. (2005) from Chapter 4. [Pavel Groisman]	
3-1977	A	93:21	93:24	Same suggestion [ILEANA MARES]	
3-1978	A	94:9	94:20	Same suggestion [ILEANA MARES]	
3-1979	A	94:35	94:36	Same suggestion [ILEANA MARES]	
3-1980	A	95:21	95:24	Same suggestion [ILEANA MARES]	
3-1981	A	97:34	97:34	Initials for Liu must be same as previous cited reference (a) [ILEANA MARES]	
3-1982	A	98:18	98:24	At line 21 the initials for Marsh are also N.D., not only N [ILEANA MARES]	
3-1983	A	98:29	98:34	My suggestion is related to the order of references [ILEANA MARES]	
3-1984	A	98:49	98:54	My notes are related to the order of references and the McCabe's initials [ILEANA MARES]	
3-1985	A	98:56		Insert two new references; McIntyre, S and R McKitrick (2003) Corrections to the Mann	

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				et al (1998) proxy data base and Northern Hemispheric average temperature series , Energy and Environment14 751-771; McIntyre, S and R McKitrick, , (2005) Hockey sticks, principal components and spurious significance. Geophys Ressearch Letters 32 L03710, doi:10.1029/2004GL1750 [Vincent Gray]	
3-1986	A	99:1	99:3	My suggestion is related to the order of references [ILEANA MARES]	
3-1987	A	99:1		Insert McKitrick R and Michaels, P J 2004 A test of corrections for extraneous signals in gridded surface temperature data.Climate Research 26 159-173 [Vincent Gray]	
3-1988	A	100:36	100:37	Updated Reference: Norris, J.R., 2005a: Multidecadal changes in near-global cloud cover and estimated cloud cover radiative forcing. J. Geophys. Res., 110, D08206, doi:10.1029/2004JD005600. [Joel Norris]	
3-1989	A	100:38	100:39	Updated Reference: Norris, J. R., 2005b: Trends in upper-level cloud cover and surface divergence over the tropical Indo-Pacific Ocean between 1952 and 1997. J. Geophys. Res. In press. [Joel Norris]	
3-1990	A	101:18	101:25	My notes are related to the order of references [ILEANA MARES]	
3-1991	A	101:32	101:43	My notes are related to the order of references and the Peterson's initials [ILEANA MARES]	
3-1992	A	101:36	101:36	Please, substitute Peterson T.R. by Peterson T.C [Manola Brunet]	
3-1993	A	101:48	101:49	Please correct the reference to: Philipona, R., B. Dürr, C. Marty, A. Ohmura, and M. Wild, 2004: Radiative forcing - measured at Earth's suface - corroborate the increasing greenhouse effect. Geophys. Res. Lett., 31, L15712, doi:10.1029/2003GL018765. [Rolf Philipona]	
3-1994	A	101:50	101:51	Please add the following reference: Philipona, R., and B. Dürr, 2004: Greenhouse forcing outweighs decreasing solar radiation driving rapid temperatur rise over land. Geophys. Res. Lett., 31, L22208, doi:10.1029/2004GL020937. [Rolf Philipona]	
3-1995	A	101:52	101:53	Please add the following reference: Philipona, R., B. Dürr, A. Ohmura, and C. Ruckstuhl, 2005: Anthropogenic greenhouse forcing and strong water vapor feedback increase temperature in Europe. Geophys. Res. Lett., 32, L19809, doi:10.1029/2005GL023624. [Rolf Philipona]	

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3-1996	A	102:24	102:24	Replaced "submitted" with "revised" [Aiguo Dai]	
3-1997	A	102:25	102:28	My note is related to the order of references [ILEANA MARES]	
3-1998	A	102:33	102:42	My notes are related to the order of references and the Randel's initials [ILEANA MARES]	
3-1999	A	103:11	103:22	My notes are related to the order of references and the Robertson's initials at line 17 [ILEANA MARES]	
3-2000	A	103:44	103:46	Same suggestion related to order of references [ILEANA MARES]	
3-2001	A	103:51	103:54	As note as above [ILEANA MARES]	
3-2002	A	103:51	:53	Delete "Rudolf et al., 1998:…" since "Rudolf et al., 1994" is more precise. [Jürgen Grieser]	
3-2003	A	104:33	104:36	Same suggestion related to order of references [ILEANA MARES]	
3-2004	A	106:21		Add the following two References: Soon, W and Baliunas, S, 2003, Proxy climate and environmental changes of the past 1000 years ; Climate Research 23 89-110, and Soon, W, et al (2003) Reconstructing climatic and environmental changes of the past 1000 years: a reappraisal, Energy and Environment, 14, 233-296 [Vincent Gray]	
3-2005	A	106:44	106:56	My notes are related to the order of references and the Sun's initials [ILEANA MARES]	
3-2006	A	107:12	107:29	My notes are related to the order of references and the Thompson's initials [ILEANA MARES]	
3-2007	A	107:37	108:8	My notes are related to the order of references and the Trenberth's initials (line 44 at page 107) [ILEANA MARES]	
3-2008	A	108:9	108:9	correct "erythrmal" to erythemal" [Reinhard Böhm]	
3-2009	A	108:11	108:11	Correct doi number: doi:10.1007/s00704-004-0034-y. [Rolf Philipona]	
3-2010	A	108:16	198:16	Correct page numbers of this reference: 417-427 (instead of 1-11). [Christian-D. Schoenwiese]	
3-2011	A	108:16		wrong page numbers: replace "1-11" by "417-427"	

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				[Jürgen Grieser]	
3-2012	A	108:24	108:24	Omit the Tyrrell (2003) reference [Dr. Nikolai Dotzek]	
3-2013	A	108:29		I have proof-read the Uppala et al. paper, but have no publication date, volume or page numbers. I'll provide an update when I can. Not sure whether it will be 2005 or 2006 now. [Adrian Simmons]	
3-2014	A	108:39	108:39	Insert: Van Wijngaarden , W.A., and L.A. Vincent, Examination of discontinuities in hourly surface relative humidity in Canada during 1953-2003, accepted, J. Geophys. Res., 2005. [Melissa Free]	
3-2015	A	108:54	108:54	Add to reference list: Vinnikov, Konstantin Y., and Alan Robock, 2002: Trends in moments of climatic indices. Geophysical Research Letters, 29 (2), doi:10.1029/2001GL014025. [Konstantin Vinnikov]	
3-2016	A	108:54	108:54	Vinnikov, Konstantin Y., Norman C. Grody, Alan Robock, Ronald J. Stouffer, Philip D. Jones, and Mitchell D. Goldberg, 2005: Temperature trends at the surface and in the troposphere. Journal of Geophysical Research, in press. http://climate.envsci.rutgers.edu/pdf/TrendsJGRrevised3InPress.pdf [Konstantin Vinnikov]	
3-2017	A	109:23	109:26	My note is related to Wang's initials [ILEANA MARES]	
3-2018	A	109:36	109:38	This manuscript has been accepted for publication in J. Climate. So, please update this reference to "Wang, X.L., V. R. Swail, and F. W. Zwiers, 2005a: Climatology and changes of extra-tropical cyclone activity: Comparison of ERA40 with NCEP/NCAR Reanalysis for 1958-2001. J. Climate, in press" (see file "StormTracksERA_NNR.pdf" on the anonymous ftp site given in Comment 1 above). [Xiaolan L. WANG]	
3-2019	A	109:38	109:38	Suggest replace "(submitted)" with "(submitted, revised according to referees' comments)". [Xiaolan L. WANG]	
3-2020	A	109:38	109:39	Add the following reference between line 38 and 39: "Wang, X. L., H. Wan, and V. R. Swail, 2005b: Observed Changes in Cyclone Activity in Canada and Their Relationships to Major Circulation Regimes. J. Climate, in press." (see also Comment #6-7 above and #21 below). See file "WangWanSwail2005_JClim.pdf" on the anonymous ftp site given in Comment 1 above). [Xiaolan L. WANG]	

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3-2021	A	109:38	109:39	Add the following reference between line 38 and 39: "Wang, X. L., and V. R. Swail, 2005: Historical and possible future changes of wave heights in northern hemisphere oceans. In: Atmosphere-Ocean Interactions - Vol. 2 [Perrie, W. (ed.)]. Advances in Fluid Mechanics Series Vol 39. Wessex Institute of Technology Press, Southampton, UK. ISBN: 1-85312-929-1, apx 300 pp." (see file "AtmosphereOceanInteractions-Vol2-Jan20.pdf" on the anonymous ftp site given in Comment 1 above). [Xiaolan L. WANG]	
3-2022	A	109:38	109:39	Add the following reference between line 38 and 39: "Wang X. L., and V. R. Swail, 2002: Trends of Atlantic wave extremes as simulated in a 40-yr wave hindcast using kinematically reanalyzed wind fields. J. Climate, 15 (No. 9), 1020-2035." (see also Comment #9-11 above). [Xiaolan L. WANG]	
3-2023	A	109:38	109:39	Add the following reference between line 38 and 39: "Wang, X. L., and V. R. Swail, 2001: Changes of Extreme Wave Heights in Northern Hemisphere Oceans and Related Atmospheric Circulation Regimes. J. Climate, 14 (No. 10), 2204-2221." (see also Comment #9-11 above). [Xiaolan L. WANG]	
3-2024	A	109:38	109:39	Add the following reference between line 38 and 39: "Wang, 2005: Climatology and Trends in Some Adverse and Fair Weather Conditions in Canada, J. Geophys. Res., in press." (see Comment #22-23 above). See file "AdverseWX95stns.pdf" on the anonymous ftp site given in Comment 1 above. [Xiaolan L. WANG]	
3-2025	A	109:38	109:39	Add the following reference between line 38 and 39: "Hanesiak and Wang, 2005: Adverse weather trends in the Canadian Arctic. J. Climate, 18 (No. 16), 3140-3156." (see Comment #22-23 above). [Xiaolan L. WANG]	
3-2026	A	109:43	109:46	My suggestion is related to the order of references [ILEANA MARES]	
3-2027	A	110:5	110:10	Same suggestion [ILEANA MARES]	
3-2028	A	111:14	111:23	Same suggestion related to order of references [ILEANA MARES]	
3-2029	A	111:40	111:51	My notes are related to the order of references and the Zhang's initials [ILEANA MARES]	
3-2030	A	111:50	111:50	Status of the paper is accepted and in press [Enric Aguilar]	

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3-2031	A	113:1	119:18	Useful section, and hopefully will answer the critics. One other criticism of thermometer measurements is the claim that as thermometers age they can show biases. To satisfy this comment, reference should be made in section 3.A.2 to the programme of regular checks done of instrument calibration. [M James Salinger]	
3-2032	A	113:1	119:1	I thought the appendix was excellent and want to emphasize that this was surely a tough section to write but I believe it strikes an excellent tone and scope in addressing key observations and methods. [Susan Solomon]	
3-2033	A	113:12	113:12	Insert following after period: "However, using a "normal" 30-year period while the climate is changing is an inadequate method for examining temperature changes. Vinnikov et al. (2002, 2004) have presented a powerful new technique for analysis and display of the diurnal and seasonal cycles of mean climate and climate change, which is insensitive to missing data and makes no requirement of the definition of a normal period. Examples show the detailed patterns of the seasonal cycle of diurnal cycle changes as well as changes of variance." ref: Vinnikov, Konstantin Y., Alan Robock, and Alan Basist, 2002: Diurnal and seasonal cycles of trends of surface air temperature. J. Geophys. Res., 107 (D22), 4641, doi:10.1029/2001JD002007. Vinnikov, Konstantin Y., Alan Robock, Norman C. Grody, and Alan Basist, 2004: Analysis of diurnal and seasonal cycles and trends in climatic records with arbitrary observation times. Geophys. Res. Lett., 31, L06205, doi:10.1029/2003GL019196. [Alan Robock]	
3-2034	A	113:12	113:13	I disagree that it is this simple. If there are absolute biases during the climatology period and the homogenisation is carried out in anomaly space then there is no guarantee that these absolute biases will have been removed as the homogenisation is only concerned with relative bias. This is especially important in radiosondes where anomaly space homogenisation is a must because there can be very large biases. Adding on the corrected series to any corrected climatology will in no way guarantee that the absolute values that result are unbiased. [Peter Thorne]	
3-2035	A	113:36	113:39	I'm not convinced that the argument stands up to scrutiny. OA has its own set of problems that should be discussed. It is not some miracle cure-all. [Peter Thorne]	
3-2036	A	113:53	113:56	The discussion of the Ordinary Least Squares trend error bars is incorrect because the a proper application of the uncertainty of the least squares fit to any function requires that the residuals from that fit are randomly distributed. Therefore, if there are autocorrelated variations other than accounted for by the linear fit, which it is stated that there are, then it	

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				is improper to assign the error bars whose definition is derived on the assumption that the fit accounts for all autocorrelation - this is a common error in the application of least square fit statistics. A proper statistical analysis should be pursued that provide for all the autocorrelated behavior of the system and assigns correct uncertainties to all parameters of the fit. - This is fundamental statistics. Nonetheless, the section as is does serve to point out how the error bars were determined, which is legitimate but maybe would be smaller if the linear fit error was isolated from the other components. [Ellsworth Dutton]	
3-2037	A	114:3		Add a sentence on robust methods: "Robust methods for the estimation of linear and nonlinear trends in the presence of episodic components became available recently and are applied to observations of temperature (Grieser et al., 2002). [Jürgen Grieser]	
3-2038	A	114:7	114:31	The authors state that most abrupt changes tend to produce random effects on regional and global trends, highlighting then that important exceptions may be changes in observation times and urban development. In my opinion this is a very important open question and it should be discussed more in detail. In particular I would suggest to acknowledge the findings of Böhm et al., 2001 who subjected about 100 secular temperature records of the Alpine area to a detailed quality control and homogenisation procedure and who performed systematic comparisons between the original and the corrected records. The results clearly display that the original series are biased by non-climatic noise and, even though the average over all the series is considered, the original data display an error of the long-term amplitude of the temperature evolution in the region of about 0.5 K. Other recent papers that support the hypothesis that regional average temperature records may be biased by systematic errors are Brunetti et al., 2005 and Begert et al., 2005. In my opinion the issue of how much averaging among station records can reduce systematic errors is one of the most important open questions in the reconstruction of climate variability and change. It should also be a basic point in the context of defining error bars of the reconstructions. In fact, as it is correctly stated in section 3.A.1. (p. 113, lines 43-44), such errors may increase the error-bar estimations shown on some of the figures displayed in the chapter. The effect is probably not so relevant at global or hemispheric level, but at a regional scale the most important contribution to the reconstruction error bars may be due to systematic biases of the station records. It may be worth noticing that where such errors have been identified and corrected, the positive temperature trend resulted significantly higher than the one obtained from the biased records. REFERENCES: Böhm R, Auer I, Brunetti M, Maugeri M, Nanni T, Schöner W. 2001. Regional	

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				Temperature Variability in the European Alps: 1760-1998 from homogenised instrumental time series. Int. J. Climatol. 21, 1779-1801. Brunetti, M., Maugeri, M., Monti, F., Nanni T., 2005: Temperature and precipitation variability in Italy in the last two centuries from homogenised instrumental time series. Int. J. Climatol., in press. Begert, M., Schlegel, T., Kirchhofer, W., 2005. Homogeneous temperature and precipitation series of Switzerland from 1864 to 2000. Int. J. Climatol. 25: 65-80. [Teresa NANNI]	
3-2039	A	114:11	114:11	after" …station relocation" add: changes in screen-type, hight above ground, observing-times and the respective algorithms to calculate daily means and others". [Reinhard Böhm]	
3-2040	A	114:11	114:11	replace "Most abrupt changes…" by "Most but not all abrupt changes…" [Reinhard Böhm]	
3-2041	A	114:11	114:12	I do not agree with this sentence: many works demonstrated that, at regional scale (but probably this is true also at larger scales), abrupt changes do not produce random effects, on the contrary, it often happen that they give a spurious signal in the long-term trend, which is sometimes comparable with the climatic signal we want to study (Böhm et al., 2001; Begert et al., 2005; Brunetti et al., 2005; Brunet et al., 2005). REFERENCES: 1) Böhm R, Auer I, Brunetti M, Maugeri M, Nanni T, Schöner W. 2001. Regional Temperature Variability in the European Alps: 1760-1998 from homogenised instrumental time series. International Journal of Climatology 21: 1779-1801; 2) Begert M, Schlegel T, Kirchhofer W. 2005. Homogeneous temperature and precipitation series of Switzerland from 1864 to 2000. International Journal of Climatology 25: 65-80; 3) M. Brunetti, M. Maugeri, F. Monti, T. Nanni. 2005. Temperature and precipitation variability in Italy in the last two centuries from homogenized instrumental time series. International Journal of Climatology, in press; 4) Brunet, M., et al., 2005: Spatial and temporal temperature variability and change over Spain during 1850– 2003. J. Geophys. Res. Submitted. [Michele BRUNETTI]	
3-2042	A	114:12	114:12	add "Auer et al., 2001" to the Vose et al., 2004 in brackets and "Böhm et al., 2001 after "…urban development" [Reinhard Böhm]	
3-2043	A	114:13	114:13	insert the following sentence after "…cooler site out of town": Böhm et al., 2001 describe the latter to be a typical situation for long-term European series preferably starting in historic centres and tending to be relocated during the 20th century to more rural airport sites. [Reinhard Böhm]	

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3-2044	A	114:17	114:17	Suggestion for re-writting this line: "When dates for discontinuities are known, a widely used approach is to compare the data for a target station with neighbouring sites (…)" [Enric Aguilar]	
3-2045	A	114:33	114:34	This sentence seems to ignore work relating urban thermal effects to night lights and work based on urban/rural differences [Dian Seidel]	
3-2046	A	114:39	114:	Recommend insertion of following text. General homogeneity adjustments routinely applied to land temperature observations should be used with caution in the analysis of individual stations. Site-specific land cover, microclimate, and instrument placement have been demonstrated (Christy, 2002; Christy et al., 2005; Davey and Pielke Sr, 2005; Gallo, 2005; Pielke Sr, 2005) to confound and override the general assumptions often used in homogeneity adjustments. [Kevin Gallo]	
3-2047	A	114:41	114:41	I would suggest to refer also to the most "to-the-point" papers included in Camuffo and Jones (2002) as it is easier for interested people to access single papers rather than the whole book. [Teresa NANNI]	
3-2048	A	114:46		Add at end. "Peterson (2003) Peterson et al (1998), Voae and Menne(2004) and Hansen et al (2001) have pointed out that the homogeneity adjustment necessary for a reliable surface record for the United States cannot be made satisfactorily anywhere else [Vincent Gray]	
3-2049	A	114:47	114:47	Please add the following paragraph: Explicitely sensitive to non climatic inhomogeneities are daily minima and maxima and consequently also DTR. They react systematically (and in most cases contrariwise) to changes in height above ground, screens and ventilation of instruments even if those do not affect temperature means. Auer et al., 2001 for central Europe could show that the systematic evolution from higher installations at north-facing walls to free-standing Stevenson screens caused the older (late 19th-early 20th century) DTR-series to be systematically too low by approximately 1 K. Brunet et al., 2005 on the other hand found original 19th century DTR-series in Spain systematically too high compared with 20th century ones. Another systematic infection of DTR-series is happening right now: the ongoing automation of measuring-networks which is typically accompanied by a change from large and unventilated screens to small and continuously ventilated ones. DTR is only one but a good example for the great care that has to be taken in terms of (in-)homogeneity of instrumental time series before going into analysis. Experience of those groups working in the homogenising business agree that it is in fact impossible to keep a long-term series clean of inhomogeneities, that all climate elements	

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				are infected and that also systematic biases are possible for larger regions. A recent respective quantitative analysis (Auer et al., 2005b) of more than 2500 detected breaks in 516 Central European series of an average length of 140 years (covering 7 climate elements) resulted in an average homogeneous subinterval of not longer than 23 years. Experience so far also tells that those inhomogeneities can be removed in most cases but also that awareness about the respective data problems (if no homogenisation is done) has not developed sufficiently within the scientific community. The painstaking work of adjusting the data-treasure that has accumulated in the climate archives so far is increasingly done by a number of research groups but it is not yet regarded a matter of course for all kind of climate variability analysis yet. Thus, one of the objectives for the future should be to continuously and increasingly invest working capacity into the respective improvement of the basis for climate change research - measured longterm climate data adjusted and thereby comparable to the present state of measuring. [Reinhard Böhm]	
3-2050	A	114:48	114:3	A version of REML, sometimes used in this chapter where uncertainty estimates are available, also takes account of the uncertainties in the data themselves e.g. global annual surface temperatures. The effect is to reduce the statistical significance of given trends, but hardly to affect the trends themselves. [Chris Folland]	
3-2051	A	115:5		Move this entire section to Line 20 Page 3-119 (the end) . All the temperature records should be together. [Vincent Gray]	
3-2052	A	115:9		Add Reference: "(Peterson and Owen 2005) and in nsert before "Rayner" , "Christy et al (2001)" [Vincent Gray]	
3-2053	A	116:47	116:57	It can be mentioned that besides the described inhomogeneities, changing sampling density (number of gaps) on different stations may affect the estimates of trends in extreme precipitation. Zolina et al. (2005) analysed the impact of missing values on the centennial-scale variability of heavy precipitation using daily data from European rain gauges and suggested a procedure for homogenization of time series. Centennial linear trends were found to be sensitive to the missing values if the number of gaps is higher than 60%. Reference: Zolina, O., C. Simmer, A. Kapala, and S.K. Gulev, 2005: On the robustness of the estimates of centennial-scale variability in heavy precipitation from station data over Europe. Geophys. Res. Lett., 32, doi:10.1029/2005GL023231.	

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				[Olga Zolina]	
3-2054	A	116:50	116:50	change "Auer, I., et al. (2005)" to "Auer, I., et al. (2005a) [Reinhard Böhm]	
3-2055	A	116:55	116:55	change "Auer, I., et al. (2005)" to "Auer, I., et al. (2005a) [Reinhard Böhm]	
3-2056	A	117:7	119:18	The next version of the CCSP vertical temperature tends report will be available for the next revision, and may modify/amplify some of these words. [Chris Folland]	
3-2057	A	117:15		What authority is there for the statement about balloon bursts? [Melissa Free]	
3-2058	A	117:24		Durre et al. involves more than just outlier checks. [Melissa Free]	
3-2059	A	118:10	118:10	Citation to documents available on NCEP Web site on assessment of NARR and GR-2 reanalysis biases over Canada may also be appropriate if they make it into literature in time. http://wwwt.emc.ncep.noaa.gov/mmb/rreanl/ [Richard Fernandes]	
3-2061	A	118:28		since late 1978 (not since 1979) [John R Christy]	
3-2062	A	118:39		Again, the original citation for 2LT is Spencer and Christy 1992. [John R Christy]	
3-2063	A	118:54	118:56	The UAH calibration coefficient is only outside of the bounds that Mears et al. thought could be possible. However, even in their latest paper (Mears and Wentz 2005) when discussing the target value, they show instances where they too have a large target coefficient for NOAA-9. Thus, I believe this statement is biased. Recall, the target value comes directly from the calculations, it is not invented, so it is an empirical result and has merit on that basis alone. Further, the NOAA-9 target coefficient difference produces almost not difference in the long term trend for 2LT and only about 40 percent for T2. The bigger differences arise at the 1991/1992 UAH v. RSS breakpoint which is where radiosondes also identify a relative breakpoint for RSS. [John R Christy]	
3-2064	A	119:7	119:13	As part of the CCSP, we have privately shown (and verified by Mears) that VG's method introduces a red herring. The more complicated non-linear equation they use is still driven by the linear hot-target value (as in UAH and RSS), so that variance reduction is not improved by adding the extra terms, and in some cases increases the error variance. The real key is that rather than calculating the hot target coefficient from time-varying relationships, VG uses space-varying relationships. In this way, VG destroy the ability of	

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				the adjustment to correct time-varying biases which can be substantial as shown in Christy et al. 2003 (Figs. 7 and 8) and in Mears et al. 2003 (Fig. 8). Thus, there result is bizarre (see CCSP, Mears has the data.). But, somehow it was published, so what can you do? [John R Christy]	
3-2065	A	119:9	119:10	Change to: However, an index of the potential destructiveness of tropical cyclones, combined over the North Atlantic and Northwest Pacific basins, shows a substantial upward trend, particularly since the 1970s… [Thomas Knutson]	
3-2066	A	119:15	119:18	This seems to me to be a statement at a much more technical level than the rest of the discussions. It is almost an afterthought and adds very little or no value to the rest of the section. Suggest that this is deleted. [Peter Thorne]	
3-2067	A	121:0	123:	Fig 3.2.1 3.2.2 3.2.3 :harmonisation of y-axis (difference / anomalies) [Jean-Marc Moisselin]	
3-2068	A	121:0	128:	Fig .3.2.1: The endpoints have some how been filtered. This will likely result in a misleading impression. The filtered curve should stop before the end points (see Benestad, 2005, GRL, doi: 10.1029/2005GL023621). [Rasmus E. Benestad]	
3-2069	A	121:0		Fig. 3.2.1 In caption (first sentence) add semi-colon: CRUTEM2v; updated … In caption (third sentence), remove "a" from "…compared with a corresponding smoothed curves …" [Melinda Marquis]	
3-2070	A	121:9	121:9	Substitute "Reynolds and Smith, 2005" with "Smith and Reynolds, 2005" [Michele BRUNETTI]	
3-2071	A	121:9	121:9	(Reynolds and Smith, 2005) is not in references, I think must be written (Smith and Reynolds, 2005) [ILEANA MARES]	
3-2072	A	121:9		delete: "a" before "corresponding" [Hartmut Grassl]	
3-2073	A	122:0	128:	Fig. 3.2.2, 3.2.4, 3.2.5, 3.2.63.2.7& 3.2.8: same as above [Rasmus E. Benestad]	
3-2074	A	122:0		Figure quality should be improved. [Henry Diaz]	
3-2075	A	122:0		Fig. 3.2.2 Improve quality of lines in graphic. Add X axis label: Year. [Melinda Marquis]	

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3-2076	A	122:0		Figure 3.2.2: what is plotted is an anomaly, relative to which mean? [Paolo Michele Ruti]	
3-2077	A	122:5	122:5	I suggest to introduce " anomaly" after temperature [ILEANA MARES]	
3-2078	A	122:6	122:7	Why "a 9 point binomial filter" in Fig. 3.2.2. But a "21 point binomial filter". [Qiang Fu]	
3-2079	A	123:0	123:	Fig. 3.2.2 is not clear. How many stations (in each category)? Which region? Anomalies with respect to what? [Rasmus E. Benestad]	
3-2080	A	123:0		Fig. 3.2.3 Add X axis label: Year. [Melinda Marquis]	
3-2081	A	123:0		Figure 3.2.3. Please state in caption what the variable is. It is only identified as USHCN data, but what physically is represented. Is it US-average surface air temperature anomalies? [Alan Robock]	
3-2082	A	123:0		Figure 3.2.3: what is plotted is an anomaly, relative to which mean? [Paolo Michele Ruti]	
3-2083	A	123:5	123:5	Define "USHCN". [Qiang Fu]	
3-2084	A	124:0		Fig. 3.2.4 In caption and Y axis labels: Are these anomalies relative to *mean* of SST from 1961-1990? Add X axis label to part (d): Year. [Melinda Marquis]	
3-2085	A	124:0		The lettering and curves in Fig. 3.2.4 are too tiny to be legible [Dian Seidel]	
3-2086	A	124:1	124:3	Figure 3.2.4 is too small to be readable [Melissa Free]	
3-2087	A	125:0		FIGURE 3.2.5. Negative signs are missing in the scale [Michele BRUNETTI]	
3-2088	A	125:0		Fig. 3.2.5 Add axes labels. Are these anomalies relative to *mean* of SST from 1961-1990? Insert "of": "...remove fluxuations of less than…" Add X axis label: Year. Add label and units to color bar. [Melinda Marquis]	
3-2089	A	125:0		The negative signs are missing in the scale of figure 3.2.5. [Teresa NANNI]	
3-2090	A	126:0		Fig. 3.2.6 In caption and Y axis labels: Are these anomalies relative to *mean* of SST from 1961-1990?	

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				[Melinda Marquis]	
3-2091	A	126:0		Figure 3.2.6: what is plotted is an anomaly, relative to which mean? [Paolo Michele Ruti]	
3-2092	A	126:5	126:5	I suggest to introduce " anomaly" after temperature [ILEANA MARES]	
3-2093	A	127:0		Fig. 3.2.7 In caption and Y axis labels: Are these anomalies relative to *mean* of SST and land surface air temp from 1961-1990? [Melinda Marquis]	
3-2094	A	127:0		Figure 3.2.7: what is plotted is an anomaly, relative to which mean? [Paolo Michele Ruti]	
3-2095	A	127:5	127:5	I suggest to introduce " anomaly" after temperature [ILEANA MARES]	
3-2096	A	128:0		Fig. 3.2.8 In caption and Y axis labels: Are these anomalies relative to *mean* of each corresponding value from 1961-1990? [Melinda Marquis]	
3-2097	A	129:0	129:	Units for both panels are identified as deg-C per century. The lower panel shows it to be deg c/decade refers to Fig. 3.2.9 [Henry Diaz]	
3-2098	A	129:0		Fig. 3.2.9 Font in graphic is too small. Change axis units to Deg C decade-1. [Melinda Marquis]	
3-2099	A	129:0		Figure 3.2.9 Grey and light blue colours are difficult to be distinguished [Javier Martin-Vide]	
3-2100	A	129:0		Figure 3.2.9 Caption Replace 'The units are ºC century-1.' by 'The units are ºC century-1 (upper) and ºC decade-1 (lower).' [Javier Martin-Vide]	
3-2101	A	129:7	129:7	The captions says units are in C/century, but the lower panel is in C/decade. I suggest having the same units, since in the current figure one gets the first impression that the trend of er the century is larger than the trend over the last 35 years. [Reto Knutti]	
3-2102	A	129:7	129:7	Units are in Celsius degree and in text in Kelvin degree [ILEANA MARES]	
3-2103	A	129:7	129:7	Units C/century : not coherent with lower figure (Deg C / decade) [Jean-Marc Moisselin]	
3-2104	A	129:7		add: "for the upper part and ?C/decade for the lower part" at the end of the sentence [Hartmut Grassl]	

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3-2105	A	130:0		Fig. 3.2.10 Font in graphic is too small. Change axis units to Deg C decade-1. [Melinda Marquis]	
3-2106	A	130:11	130:11	linear trend of seasonal MAM, JJA, SON and DJF temperature trends" : "linear trend of seasonal MAM, JJA, SON and DJF temperature [Jean-Marc Moisselin]	
3-2107	A	131:0		Fig. 3.2.11 Font in graphic is too small. Change axis units to Deg C decade-1. [Melinda Marquis]	
3-2108	A	131:0		Figure 3.2.11 Regions with incomplete or missing data are so numerous that this map could be removed. [Javier Martin-Vide]	
3-2109	A	131:0		Suggest stating what fraction of the global surface area is colored gray (missing data) in Fig. 3.2.11 caption. [Dian Seidel]	
3-2110	A	131:2	131:6	comment: could be omitted, small text sufficient" [Hartmut Grassl]	
3-2111	A	132:0	132:	This is figure is somewhat confusing as it has too many lines. They should select the green bars plust GHCN smoothed, CRU smoothed and one but not both of the GPCC curves. [Henry Diaz]	
3-2112	A	132:0	132:	inconstitency figure and legend for the base period mean [Jean-Marc Moisselin]	
3-2113	A	132:0		Figure 3.3.1. Legend – GPCC VASClimO instead of GPCC VasClim [Christoph Beck]	
3-2114	A	132:0		FIGURE 3.3.1. In the figure title the curve is defined as the deviation from 1981-2000 mean, while in the figure caption it is defined as the deviation from the 1961-1990 mean [Michele BRUNETTI]	
3-2115	A	132:0		what is the base period for these anomalies, 1961-1990 or 1981-2000. Please check the captions. [Annarita Mariotti]	
3-2116	A	132:0		Fig. 3.3.1 In caption: Are these anomalies relative to *mean* ppt value from 1961-1990? [Melinda Marquis]	
3-2117	A	132:0		There is inconsistency between the title and the caption of figure 3.3.1. [Teresa NANNI]	
3-2118	A	132:0		Is the use of green and red in Fig. 3.3.1 problematic for color-blind readers? [Dian Seidel]	
3-2119	A	132:5	132:5	Anomalies with respect to the 1961-1990, but in the inside of the picture there is another	

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				base period 1981-2000 ?? [ILEANA MARES]	
3-2120	A	132:6	132:6	suppress "(to convert…366)" that seems trivial [Jean-Marc Moisselin]	
3-2121	A	132:7	132:7	Reference for GPCC VASClimO (Beck et al. 2005) should be added [Christoph Beck]	
3-2122	A	133:0	133:	Bottom panel of Fig. 3.3.2: There should be enough reliable data in Mexico for this region to show valid trend values. What is the size of the grid boxes used in this figure? [Henry Diaz]	
3-2123	A	133:0		Fig, 3. 3.2 Change axis units to % century-1 and % decade-1. [Melinda Marquis]	
3-2124	A	133:0		Figure 3.3.2 Caption Replace 'The units are % century-1.' by 'The units are % century-1 (upper) and % decade-1 (lower).' [Javier Martin-Vide]	
3-2125	A	133:0		In Fig. 3.3.2 caption, the units are given as %/century but the bottom panel shows %/decade. Also, the caption doesn't make clear what is meant by "valid" years. [Dian Seidel]	
3-2126	A	133:0		Figure 3.3.2 The scale on this seems non-sensical. How on earth can you have a trend of <-100%? Surely once you've lost 100% of your rain it doesn't start raining spontaneously from the ground to the sky? Suggest this is clarified. [Peter Thorne]	
3-2127	A	133:5	133:5	Units %/century : not coherent with lower figure (% / decade) [Jean-Marc Moisselin]	
3-2128	A	134:0		Fig. 3.3.3 Is a bit fuzzy [Melinda Marquis]	
3-2129	A	134:0		Fig. 3.3.3 is too small to be read. [Dian Seidel]	
3-2130	A	135:0		The weighting functions of T_trop-UW and T_mid-trop, which are biased, should be redrawn based on actual data. [Qiang Fu]	
3-2131	A	135:0		Fig. 3.4.1 Font a bit small. [Melinda Marquis]	
3-2132	A	135:0		I don't like the term "dividing line" to define the tropopause, which is probably more of a region or layer than a distinct line. Maybe "separation" is a better choice. What is the purpose of the dashed lines at 300 and 100 hPa? [Dian Seidel]	

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3-2133	A	135:9	135:9	Add "global-mean" before "the troposphere as a combination". [Qiang Fu]	
3-2134	A	136:0		A curve is misssing in the 3rd pannel (black line). [Qiang Fu]	
3-2135	A	136:0		Fig. 3.4.2 Font a bit small, fuzzy. Add axes labels: X is year; Y is Temp anomalies relative to mean temp of 1979-1997. [Melinda Marquis]	
3-2136	A	136:0		The caption for Fig. 3.4.2 might clarify that the volcanoes indicated are highly explosive ones that injected aerosols into the stratosphere that lingered there. Otherwise one might infer that these are the only volcanic eruptions during the period. [Dian Seidel]	
3-2137	A	136:1	136:3	Figure 3.4.2-- Why does the NOAA radiosonde series seem to have gaps in it in the lower troposphere panel? [Melissa Free]	
3-2138	A	137:0		Fig. 3.4.3. I very much like this figure because of the considerable amount of information rendered, but I do have three suggestions. First, in virtually all color representations, UAH has been designated blue (school color) and RSS red (so first letter of color matches source – as is done in 3.4.3). The second is the issue of error bars. I think it is important to get across to the reader that at present trends such as RSS and UAH are significantly different. This is determined by comparison-testing of the difference time series in which common variability is removed. As is, the figure gives the impression that the datasets are not significantly different – a false impression –due to the particular variability contained in the 1979-2004 period which both datasets observe. Thirdly, it is false to say Fu et al. remove stratospheric influences. As Spencer, Christy, Braswell and Norris (2005, JTech) show, it is impossible to remove the stratosphere using only MSU 2 and 4. Rather, the Fu retrieval changes the stratospheric influence in the hopes it is statistically minimized (but then the problem of non-stationarity comes into play.) [John R Christy]	
3-2139	A	137:0		Fig. 3.4.3 Change units K per decade. [Melinda Marquis]	
3-2140	A	137:0		Figure 3.4.3: Doesn't this have to be updated to correct for the latest UAH error, in the sign of the warm-target correction? [Alan Robock]	
3-2141	A	137:0		Figure 3.4.3 The use of trend error bars gives an entirely mis-leading impression that everything agrees. But these error bars are dominated by the common high frequency variance. Use of proper statistics like uncertainty in trends in the difference series highlights residual problems in the data (see the CCSP report appendix). You can caveat	

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				all you like in the text, but the figure will be lifted and used in isolation and hence I urge usage of appropriate error statistics which actually answer the questions most naive non-experts will be expecting them to answer which are: Is dataset A different to dataset B or is level X different to level Y? The use of the current error bars for this purpose would be entirely misleading and lead to too many conclusions that there are no differences between datasets. CCSP denoted trend significance by filled symbols for this reason and did not place error bars so as to avoid this potential mis-/over-interpretation. Reasons for this choice are expanded in the appendix of this report. [Peter Thorne]	
3-2142	A	137:5		I find this diagram to be very confusing/ You are displaying not only trends, but the 95% error range of each trend, and you should explain that at the beginning. [Vincent Gray]	
3-2143	A	138:0		Fig. 3.4.4 The choice of the simple retrieval for RSS here is probably not wise. First, it is a retrieval that compounds the errors of the dependent datasets. Secondly, this uses RSS which appears to have a significant warm bias due to a shift at the end of 1991. If this remains, the authors' will need to provide language to account for such a possibility (i.e. this depiction may contain significant error) and give themselves some cover from potential future criticism. The smoothness of the field is also very suspicious. If RSS is preferred, I would suggest the direct LT representation and thus avoid the pitfalls of a simple retrieval (which can never be true for individual locations as displayed in a map like this.) If any bias is detected in this chapter, it is with this choice of picking the warmest realization in terms of trends that is not consistent with many other datasets. Please be careful. [John R Christy]	
3-2144	A	138:0		Fig. 3.4.4 font a bit small [Melinda Marquis]	
3-2145	A	138:0		Figure 3.4.4 I am very unhappy at this figure. It would be much wiser to use 2LT and plot UAH, RSS, and the difference field. At the very least RSS 2LT would be a more sensible diagnostic. Fu et al was trained on very large areas and never claimed to yield grid-box resolution troposphere-only data, certainly in the earlier papers. Taken together with the existing text this ability of the technique is strongly implied. More pertinent information could be gained by presenting 2LT maps instead. [Peter Thorne]	
3-2146	A	138:1	138:3	Figure 3.4.4-- The figure would be more informative if a different scale were used. [Melissa Free]	
3-2147	A	138:2		comment: "White Interval" does not exist [Hartmut Grassl]	

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3-2148	A	139:0		Fig. 3.4.5 Is 1.2 % per decade a result (interpretation) of data in graph? If so, probably belongs in chapter text rather than in fig. [Melinda Marquis]	
3-2149	A	139:0		I'd suggest switching the colors on Fig. 3.4.5. Making blue represent moistening would be more intuitive than having it represent drying. [Dian Seidel]	
3-2150	A	140:0		Fig. 3.4.6 Could you use red font for right-side Y axis label, for ease of understanding? Label X axis: Year. [Melinda Marquis]	
3-2151	A	140:0		Fig 3.4.6 : the base period mean for anomalies is not indicated [Jean-Marc Moisselin]	
3-2152	A	140:0		Why are Canada and the US excluded in Fig. 3.4.6? [Dian Seidel]	
3-2153	A	141:0	141:	Fig 3.4..7 Why is the sign of the net radiative anomaly flipped compared to the other two graphs? Shouldn't it just be the sum of the LW and the SW? [Nathan Gillett]	
3-2154	A	141:0		3.4.7 Axis labels too big. [Melinda Marquis]	
3-2155	A	141:0		Fig 3.4.7 : the base period mean for anomalies is not indicated [Jean-Marc Moisselin]	
3-2156	A	142:0	142:	Figure 3.5.1 Why is only a plot of 200 hPa height trends included for the sections on SLP and geopotential height changes? Most of the discussion in 3.5.2 focuses on changes lower in the troposphere, yet no plots of lower tropospheric geopotential height or SLP trends are shown. [Nathan Gillett]	
3-2157	A	142:0	142:	The figures have not indications for (a),(b),(c) and (d) [ILEANA MARES]	
3-2158	A	142:0		Figure 3.5.1 has no published reference either, and although the current version for the IPCC now was created by James Renwick, we (Bromwich and Fogt) created the original figure. [David / Ryan Bromwich / Fogt]	
3-2159	A	142:0		Fig. 3.5.1 Add labels a, b, c and d to graphic. [Melinda Marquis]	
3-2160	A	143:0		Box 3.2 Fig. 3.5.2 Add right and left Y axes labels. Move Y axes units (hPa and km) so that they don't interfere with Y axes values. What is white vs. unshaded?	

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				[Melinda Marquis]	
3-2161	A	143:0		Fig. 3.5.2 caption doesn't explain the color scheme. [Dian Seidel]	
3-2162	A	143:4	143:9	Taken together with the accompanying text in the main chapter I cannot make head or tail of this. The caption needs to be significantly improved. [Peter Thorne]	
3-2163	A	144:0		In Fig 3.5.3, is there some way to distinguish regions of non-significant trends from regions with insufficient data? [Dian Seidel]	
3-2164	A	145:0		Fig. 3.6.1 Increase font size in graphic. [Melinda Marquis]	
3-2165	A	145:0		Suggest enlarging the + sign in Fig. 3.6.1. [Dian Seidel]	
3-2166	A	145:0		Figure 3.6.1 Its almost impossible to see the crosses denoting the grid-points these are centred on. Can these be made, bigger, bolder, and much clearer? [Peter Thorne]	
3-2167	A	146:0		Fig. 3.6.2 Increase font size in graphic. Add label to X axis (Year) for bottom right panel. [Melinda Marquis]	
3-2168	A	147:0		Fig. 3.6.3 In top graphic, what is 1:20.8% refer to? In bottom graphic, add label to X axis (Year). [Melinda Marquis]	
3-2169	A	147:1	147:11	Figure 3.6.3 (a)The SST anomaly EOF is shown only for the PDO. As it is a joint ocean/atmosphere phenomenon the SLP pattern associated with the PDO should ber shown as well, as was shown for the SO index. [M James Salinger]	
3-2170	A	147:3	147:9	Figure 3.6.3 (b). The time series for the IPO as well would be usefully shown to demonstrate the linkages, and that this is probably a basin wide phenomenon., [M James Salinger]	
3-2171	A	148:0	148:	Figure 3.6.4: description of the index in the lower panel not clear. It states that the record has been "inverted". Perhaps it would be more straightforward to state that it was multiplied by a minus 1 for easier comparison with the upper panel. A sentence in the caption that notes the increase in tropical Indian Ocean SST in concert with lower SLP in the Aleutian region would be helpful. [Henry Diaz]	
3-2172	A	148:0		Fig. 3.6.4 Add label to X axes (Year). Remove "-" that precedes "Indian SST" in upper right of bottom graphic.	

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				[Melinda Marquis]	
3-2173	A	148:1		Lower panel of figure 3.6.4. In presenting the inverted trend for Indian SST it might be clearer to invert the vertical axis than plotting the inverted Indian SST data. The caption would specify that the vertical scale is inverted. [Philippe Tulkens]	
3-2174	A	148:11	148:11	Why this assymetrical definition of what the tropical indian ocean region is (10S to 20N)? [Peter Thorne]	
3-2175	A	148:13	148:13	"with (a)" is referred to the picture of the top ? [ILEANA MARES]	
3-2176	A	149:0		Fig. 3.6.5. The beginning of the caption is not clear (to me). The "unit deviation" is the actual NAO index average variation in the period 1900-2005? [Franco Desiato]	
3-2177	A	149:0		Fig. 3.6.5 Increase font size in lettering in graphic. [Melinda Marquis]	
3-2178	A	149:9	149:13	Values of SLP, SST and precipitation are multiplied with 1/10, but in the inside of the picture the indication is that these values are multiplied by 10hPa,10C and respectively 10mm ?? [ILEANA MARES]	
3-2179	A	150:0		Fig. 3.6.6 Add X axes labels and units: Time (Year). Add Y axis units. Clarify Y axes labels: Are these sea level pressure anomalies? Probably don't want to include statement to see URL for updated time series in caption. [Melinda Marquis]	
3-2180	A	150:12	150:12	" Atlantic-sector" My suggestion is to give the borders of this sector, if it is possible [ILEANA MARES]	
3-2181	A	151:0		Fig. 3.6.7 Increase font size in graphics. In caption, describe the three parts in order, either top to bottom, or bottom to top. Add units to Y axis. [Melinda Marquis]	
3-2182	A	152:0		Fig. 3.6.8 Improve quality of lettering in graphic. In caption, insert word (bottom) after "SST anomalies." Revise Y axes labels to refer to *mean* temp for 1961 *to 19*90 [Melinda Marquis]	
3-2183	A	153:0	154:	Figures 3.7.1 and 3.7.2 suggest weakening summer monsoon rainfall in most places. It would be useful to show a combined index of the respective monsoon rainfall in the NH and SH as well as an overall area-weighted index. [Henry Diaz]	
3-2184	A	153:0	154:	Figures 3.7.1 and 3.7.2. Is the difference in pre-1975 and post-1975 precipitation in the NCEP reanalysis really reliable? My understanding is that precipitation in the reanalysis	

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				is not particularly reliable in general, and that discontinuities associated with the assimilation of satellite data from 1979 are also likely to be found. Couldn't the CRU precip data be used instead? [Nathan Gillett]	
3-2185	A	153:0		Fig. 3.7.1 Add label and units (mm day-1) to colour bar. [Melinda Marquis]	
3-2186	A	153:0		Figure 3.7.1. Caption Replace 'rage' by 'range' [Javier Martin-Vide]	
3-2187	A	153:0		Figure 3.7.1 Colour scale is inverted compared to "naïve" expectation that green / blue would be wetter and red/ yellow drier. The scheme may want to be switched to make the figure more intuitive. [Peter Thorne]	
3-2188	A	154:0		Fig.3.7.2 Fig to be redone. When redone, please label X and Y axes. Improve quality of lettering in graphic. [Melinda Marquis]	
3-2189	A	155:0	155:	The caption should state how the index was calculated. It should be defined based on a cited reference. [Henry Diaz]	
3-2190	A	155:0		Fig. 3 7.3 Add X axis label: Year. [Melinda Marquis]	
3-2191	A	156:0		Fig .3.7.4 Add X axis label: Year. [Melinda Marquis]	
3-2192	A	156:7	156:7	Change 2003/2004 to 2004/2005. This figure was updated after the zeroth-order draft, and the figure caption needs to be updated accordingly. [Matthew C. Wheeler]	
3-2193	A	157:0		Fig. 3.7.5 Add X axis label: Year. Add units to Y axis. [Melinda Marquis]	
3-2194	A	158:0		Fig. 3.8.1. Since the meaning of the the two diagrams strongly depend on the choice of the three time intervals (01-50; 51-78; 79-03), it would be worth to include in the caption a short note on the reasons of this choice (even if it is arbitrary) [Franco Desiato]	
3-2195	A	158:0		Figure 3.8.1 I didn't understand this diagram. The caption says 'The x-axis represents the percentage of time during the year when the indicators were below the 10th percentile for cold nights….'. Surely for a single station in the climatology period, the cold night temperature is below the 10th percentile 10% of the time over the whole period. Is this sampled over individual years? More explanation in the caption would be helpful.	

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				[Nathan Gillett]	
3-2196	A	158:0		Fig. 3.8.1 Add X and Y axis labels. Remove "202 stations" from each graphic. [Melinda Marquis]	
3-2197	A	158:10		Figure 3.8.1: Caption, Line 10, should reference Alexander at al. (2005a), not (2005). [John Caesar]	
3-2198	A	159:0	159:	Fgiure should be identified by (a)--which according to the description is comprised of the top two panels; and (b) for the lower panel. [Henry Diaz]	
3-2199	A	159:0		Figure 3.8.2 The caption to the lower panel states that 'Regions where disproportionate changes in heavy and very heavy precipitation were documented compared to the change in the annual or seasonal precipitation'. Presumably the + signs therefore indicate places where the heavy and very precipitation changed disporportionately more than the mean, and - signs regions where it changed disproportionately less than the mean? Or do + signs represent regions where the heavy events increased by a disproportionate amount, and minus signs regions where they decreased by a disproportionate amount? This needs to be clarified. Also I find the diagram difficult to interpret, because it is hard to assess how large the regions with disproportionate changes are compared to those with proportionate changes (which presumably are the regions between the symbols). This is information is required to assess the global significance of the changes. [Nathan Gillett]	
3-2200	A	159:0		Fig. 3.8.2 Figures are a bit fuzzy. Label two graphics (a) and (b) to correspond to caption. Heading for part (a) refers to "Trend per % decade" but caption refers to "trends (%) per decade." Revise heading to match caption. Label units of color bar. Label X and Y axes of both graphics in top part (A). Caption for (b) states that definitions for "heavy" and "very heavy" vary by season and region; this should probably be in text of chapter, not in caption. Next sentence in caption, "However, changes in ..." should probably also be in chapter text, not in caption. [Melinda Marquis]	
3-2201	A	159:0		Figure 3.8.2 Why are some countries coloured in on the bottom panel but others not? This is not explained either in the text or the figure caption. [Peter Thorne]	
3-2202	A	160:0		Fig. 3.8.3. Font in graphic is too small. Should be able to read axis labels and units of graphs in graphic. [Melinda Marquis]	
3-2203	A	160:0		The timeline and ACE axis labels are too small to be legible in Fig. 3.8.3 [Dian Seidel]	

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3-2204	A	160:1	160:3	Figure 3.8.3 is illegible because it is too small. [Melissa Free]	
3-2205	A	160:1	160:1	Figure 3.8.3. The timelines are illegible, as is the vertical axis. May consider also showing a figure from Emanuel (2005)--or better yet, contact Emanuel about an updated figure through 2005 season. [Thomas Knutson]	
3-2206	A	160:7	160:7	I believe you probably mean to say "The range of the vertical scale in the West N. Pacific is twice that of other basins." [Thomas Knutson]	
3-2207	A	161:0	161:	The description of Fig. 3.8.4 is not at all clear. It is defined as "95/99 percentiles of standardized geosptropic winds…" The Y-scale values appear to be starn standardized units, but the two descriptors appear ackward. [Henry Diaz]	
3-2208	A	161:0		Fig. 3.8.4 Graphic is a bit fuzzy. Add X and Y axis labels. Include definition of units on Y axis. [Melinda Marquis]	
3-2209	A	162:0		Box 3.5.3, Fig. 3.8.5 Clarify caption by stating fully the second class: moderate to extreme wet [Melinda Marquis]	
3-2210	A	162:0		Are both panels needed in Fig. 3.8.5. The are highly anticorrelated. [Dian Seidel]	
3-2211	A	163:0		Figure 3.8.6, lower panel. This panel appears to compare a Gaussian fitted to 1961-1990 JJA maximum temperatures with a Gaussian fitted to 2003 temperatures. Since Gaussians have been fitted to the distributions, this diagram can tell us nothing more than the difference in means and the differences in variances. And because of interannual variability, we would expect the variance of maxima from multiple years to be larger than that from a single year - therefore the variances of the two curves are not directly comparable. This leaves only the change in the means, which doesn't seem worth including a plot for. [Nathan Gillett]	
3-2212	A	163:0		Box 3.6.7, Fig. 3.8.6 In top graphic heading and caption, clarify that these values are compared to *mean* temp for period 1961-1990. Add X and Y axis labels to top graph. Add units to Y axis. [Melinda Marquis]	
3-2213	A	163:0		Fig.3.6.7, lower panel: I doubt that this panel makes sense, as it compares pdf-distributions of a 30-year maximum temperature distribution against a pdf of a single	

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				year. For a single year, the pdf is poorly approximated by a Gaussian distribution. If the purpose of the figure is to demonstrate the extreme conditions in 2003, it might be better to use Fig.1e of Schar et al (2004). Also, the special aspect of this summer was less its extreme maximum temperature (although some Tmax record values were recorded), but rather the long duration of the heat wave (see Black et al. 2004). [Christoph Schar]	
3-2214	A	163:0		Box 3.6.7 Fig. 3.8.6 The summer of 2003 was amazing as shown by the upper panel of this figure. But I do not understand the lower panel. How can you compare the distribution over 30 years with the "distribution" of a single summer? This seems like a meaningless comparison. [Steven Sherwood]	
3-2215	A	164:0		Fig. 3.9. 1Need to provide a figure caption (other than what's there now: "Currently these are from the TAR.") [Melinda Marquis]	
3-2216	A	164:4	164:4	In updated versions of Fig. 3.9.1, the text "? no systematic large-scale change in tornadoes, thunder-days, hail" should be split up in two separate labels, one for "thunderstorms and lightning", and one for "tornadoes, damaging winds, hail". [Dr. Nikolai Dotzek]	
3-2217	A	164:6	164:6	If Fig. 3.9.1 remains in chapter 3, then chapter 10 should have a similar one with future projections [Reto Knutti]	
3-2218	A	165:0		Question 3.1, Fig.1. In the figure caption it should be specified the meaning of the black dots in the diagram, hether they represent stations or grid data and how they have been selected. [Franco Desiato]	
3-2219	A	165:0		Question 3.1, Figure 1: The exact choices of trends shown are arbitary, and give the impression that something within the climate "switched on or off" at those points. It would be much better not to join up the "smoothed depiction" for each period but rather leave them as disjointed lines to show how the trends change but without infering distinct turning points. Surely by "forcing" the period trends to join up that will limit the trends that can be deduced? [Gareth S. Jones]	
3-2220	A	165:0		Q. 3.1 Figure caption for top graphic should probably refer to the Y axis label, which should state that it's the *mean* of temps for period 1961-1990. Break the third (last) sentence about top graphic into three sentences: one for each time period described.	

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				Actually, since this third sentence describes interpretation of data in graphic, it probably belongs in text of chapter, not in figure caption. Font on bottom graphic (both left and right) is too small. [Melinda Marquis]	
3-2221	A	165:0		The color scale labels are illegible in Fig. 1. Also, the caption should be more explicit in giving the source of the "satellite records" used for th trend maps. [Dian Seidel]	
3-2222	A	165:0		Question 3.1, Figure 1. Two points. Firstly the bottom right hand panel should be one of RSS or UAH TLT and the caption should note the large residual uncertainty in tropospheric trends. Much more importantly the top panel is very very easy pickings for those who wish to discredit climate science / the IPCC process. You are showing 95% error bars that contain only approximately 2/3 of all points (and only about half from the mid-20th Century). My expectation is that many people will use only this figure from all that has been so diligently prepared by the authors and particularly the coordinating lead authors for this chapter. Please reconsider EXTREMELY carefully how this information is presented. And remember that probably the majority of uses will not keep the caption but simply lift the figure and use that in isolation, so the figure on its own must be comprehensible and inclusive. It certainly should not have much more than 5% of points outside of 95% CIs for this reason. [Peter Thorne]	
3-2223	A	165:7	165:7	My suggestion is that after "temperatures" to introduce anomalies [ILEANA MARES]	
3-2224	A	166:0		This figure is difficult to follow. [Qiang Fu]	
3-2225	A	166:0		Q. 3.2 Fig. 1 Label units on color bars. Label X axis on bottom graphics: Year. Add units to Y axis of bottom graphic. [Melinda Marquis]	
3-2226	A	166:0		Question 3.2, Figure 1. Are the figures in the lower panels the percentage of variance explained by these patterns? If so then these don't seem important enough to include in this report. What do we learn from these? That droughts vary in space? That much one can say in as many words. [Steven Sherwood]	
3-2227	A	166:6	166:12	The caption for Q3.2 figure 1 now makes an excellent job of describing the diagram - we thank the authors for addressing the points we raised about this in their initial draft of the question. One important addition however is to add a definition of the PDSI in the text or caption.	

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				[David & David Wratt & Fahey]	
3-2228	A	167:0		Q. 3.3 Fig. 1 In caption, clarify that these values are compared to *mean* temp for period 1961-1990. Label all X and Y axes, e.g., year, latitude, longitude. Label four parts of graphics (a), (b), (c), and (d). [Melinda Marquis]	
3-2229	A	167:5	167:5	Figure 1 : "trends (days) per decade" : "trends (days per decade)" [Jean-Marc Moisselin]	
3-2230	A	167:5		Suggest explaining or omitting 'percentile temperature differences' and '21-term binomial fit' in order to simply caption. [David & David Wratt & Fahey]	
3-2231	A	167:8		Question 3.3, Figure 1: Caption, line 8, should read "Below each map are the global annual time series anomalies", not "…is the global annual time series anomalies..". [John Caesar]	